THURSDAY, JANUARY 19, 1871

THE MEDITERRANEAN ECLIPSE, 1870

CLOUD in Sicily, cloud in Spain, cloud in Africa. Such at first sight might seem to be the only result of all the observations made on the eclipsed sun of 1870; such the reception given by Nature to those who wooed her as she had never been wooed before, who approached her full of the rarest gifts which Science has placed at man's disposal.

But, after all, has the oracle been silent? I think not. Dare we, however, say that the great problem of the Corona, that one among the many still outstanding difficulties which the eclipse was invoked to settle, is settled? This, perhaps, would be saying too much, but still, I think, a step in advance has been made. The oracle has spoken darkly perhaps, but it *has* spoken.

Let me endeavour to put the question as it stood a few weeks ago as briefly as possible.

Beginning the story some few years back we find the corona, a halo of white light round the moon, with a height sometimes represented as equal to the moon's diameter, sometimes more, sometimes less, with a border *à discrétion*, —so much did the drawings vary—regarded as the solar atmosphere.

Some thought the red prominences to be mountains, other observers called them clouds.

The polariscope was brought up with a view of determining whether the corona shone by reflected light or not. The result of this new method of observation was doubtful.

In the Indian eclipse of 1868 M. Janssen, by means of the spectroscope, still another aid, determined that the prominences were masses of hydrogen gas, but there was no final word about the corona. Major Tennant observed that its spectrum was continuous. Later in the same year Dr. Frankland and myself approximately determined the pressure of the prominence gases by means of the new method and laboratory experiments, and at once stated our conviction that the extensive corona which had been depicted and represented by Kirchhoff and others to be the solar atmosphere must be something else. This was our idea. I cannot quote our words, for I am writing in Venice and have no copies of our paper with me.

In the American Eclipse of 1869 the problem was advanced considerably, perhaps even more considerably than we can yet form an idea of, writing as we must still do doubtfully. I do not refer to the drawings, for they varied considerably, but to the observation that the light of the outer corona, like that of the prominences, gave a bright-line spectrum. But as at least some of the observers gave positions doubtfully, "near C" and "near E," I thought that the explanation was still possible which regarded the corona as of terrestrial origin; that is, which assumed it to be an appearance due to the presence of light in our own atmosphere. The problem was one of such difficulty that there seemed a possibility that, by some unexplained cause, some of the solar light might be diffused and beat out of its course, and then, mixing up with the light of the chromosphere, give us a sort of continuous spectrum, with the hydrogen bright lines superposed upon it; in other words, that as the eye perceives a bright, irregular region or glare around the uneclipsed sun, an effect due to our atmosphere, so also the eye might perceive a bright, irregular region or glare round the *uneclipsed chromosphere during eclipses*, due also to our atmosphere.

One word here about the Chromosphere, the name I have given to the bright-line-giving region outside the photosphere. It has long been clear that the spectroscopic method of observing it when the sun is not eclipsed is not totally effective; that is to say, that we only see a per-centage of it—perhaps only a relatively small per-centage—but the glowing prominences, that is, those in which there is no evidence of the rapid motion of ejection from the sun, the ejection taking place at all angles from the line of sight, afford evidence that there is probably a layer of cooler hydrogen susceptible of being rendered visible above the ordinary level. Now as these prominences may be 5′ high, it is not unreasonable to suppose that the chromosphere may even extend to that distance, or even a little beyond it.*

Hence it was that in the Instructions to Observers, drawn up by Professor Stokes and myself, and approved by the Organising Committee for this 1870 Eclipse, it is stated that—

"The PRINCIPAL OBJECT to be obtained is to determine whether it is possible to differentiate the outer layers of irregular outline and the streamers (of the corona) from a stratum, say some 5′ or 6′ high, round the sun, which may possibly be the limit of the gaseous envelopes above the photosphere."

The spectroscopic observers, therefore, were enjoined—

a. "To determine the actual height of the chromosphere as seen with an eclipsed sun; that is, when the atmospheric illumination, the effect of which is doubtless only partially got rid of by the Janssen-Lockyer method, is removed. If the method were totally effective, the C line, the line of high temperature, should hardly increase in height; but there can be little doubt that the method is not totally effective, so the increase in height should be carefully noted."

b. "To determine if there exists cooler hydrogen above and around the vividly incandescent layers and prominences."

And the polarisers—

"To examine a detached and selected part of the corona about 6′ from the limb of the sun, and say about 8′ in diameter."

* Here is what I wrote on this point a year ago:—"I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere. This is considerable; in fact, the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—'I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D, and F) before and after totality, and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*'" This effect was observed with two flint prisms and 7 inches aperture. Professor Young, with five prisms of 45° and 4 inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality. He writes:—'I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly*, but 2796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness.' These observations I consider among the most important ones made during the eclipse; for they show most unmistakeably that, as I have already reported to the Secretary of the Government-Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion." (Proc. R. S. 1870, p. 181.)

Having got so far, it may be here stated that of the three means of attack, namely, the spectroscope, the polariscope and telescope, and naked eye observations, the spectroscopic method, under certain circumstances, might have been by far the most doubtful, the polariscope method coming next.

With regard to the spectroscopic observations, if we assume that no light whatever is received by and from our own atmosphere, the observatiors would be easily translated. A pure continuous spectrum would reveal to us solid or liquid matter in the circumsolar regions; a spectrum continuous or not containing bright lines would give us gases or vapours; the ordinary solar spectrum, with its dark lines, would indicate matter incapable of radiation itself, and therefore cool, reflecting to us ordinary sunlight. It is clear that the problem would be complicated if circumsolar matter both reflected sun light and sent us its own; and still more so if we allow that the coronal light may be partly contributed from reflections and refractions in our own atmosphere. Then we have to consider whether the light thus contributed may possibly be due to the photosphere or to the prominences, and we are landed in a maze of difficulties which need not be discussed here.

The system of sketching introduced for this eclipse is at once so simple and final that the only wonder is it has not been introduced before. The corona must be either solar, atmospheric, or subjective, that is, more or less built up in the observer's eye, this more or less depending *cæteris paribus* upon the brilliancy of the undoubted solar portion. If at all stations, the stations being as wide apart as they have been this time, the drawings are similar, then the corona would be undoubtedly cosmical; if dissimilar, then it would either be terrestrial or subjective: and this point could and would have been settled this time, if the weather had permitted, by arranging the observers *in pairs*, that is, dealing with two observers in each place instead of a single one, and so obtaining the eye-variation.

This being premised, what is the result of the very few observations, comparatively speaking, which have been made? Before I attempt to give any idea of my answer to this question, it is only fair to myself to state that my only sources of information, up to the present time, have been conversations with some of the American members of the Sicilian expedition, a brief telegram from the members of the English party at Agosta, the Rev. S. J. Perry's communication to the *Daily News* of the 2nd instant, and an inspection of some drawings made by the officers of H.M. ships off Aci Reale. At Catania we saw a portion of the corona for $1\frac{1}{2}$ seconds through a cloud, and that was all; and the day after the eclipse, before the more fortunate members of my party returned, it became my duty to proceed to Malta in H.M.S. *Lord Warden* to attend the court-martial on the officers and crew of the beautiful, but unfortunate, *Psyche*, in which we had been wrecked on the 15th ult., and the weather in the Mediterranean has been so bad that it was impossible to leave Malta in time to rejoin the expedition before they left for England. Of detailed information, therefore, I have none.

In the first place, then, I submit that the fact that the corona is a compound phenomena comes out in an unmistakable way. We have first of all a ring some 5′ or 6′ high round the moon, which almost all observers have seen alike; and then we have light beyond which some observers have seen of one shape and some of another, now stellate with many rays, now stellate with few, now absolutely at rest, now revolving rapidly.

This I think to be the key-note of all the observations with which I have become acquainted. I need scarcely say that it is exactly what had been predicted.

First among the fortunate ones who observed the corona with the telescope was Prof. Watson, of Ann Arbor, who took up his station at Carlentini, and appears to have been the best favoured among the Sicilian observers. From his account I gather that there was an almost perfect *shell* around the sun about 5′ high, and that outside this shell were less definite rays. What he was particularly struck with was this, that, as seen in the telescope, the rayed portion was most developed over the prominences, and, as I gathered from him in one case, the rayed portion was absent as if a veil had been removed; so that he, at all events, is strongly impressed with the idea that the shell represented a true solar appendage, and that the rayed structure was due to our own atmosphere.

Next comes Mr. Brett, who, although he was not so fortunate, still was enabled to see and place on record some most interesting features, including the whole outline of the corona and even some of the protuberances. He also, as I am informed, saw the rayed portion of the corona most developed above the protuberances, the outline of the interior portion being visible, though not so strongly marked as in the case of Prof. Watson's drawing, in consequence of less favourable atmospheric conditions. I am thankful to say that the weather at Syracuse enabled Mr. Brothers to obtain some admirable photographs, which I have not yet seen. These are among the most important results of the Expedition.

Next I must mention Prof. Peirce, the head of one of the American parties, who observed two miles north of Catania, at a private casino of the Marchese Sangiuliano. I believe that he also saw the shell, but of this I am not absolutely certain; but he distinctly observed that the outer corona over the prominences was rosy red, although he did not see the prominences himself. A more beautiful proof of the terrestrial nature of this portion of the corona it would be difficult to imagine: for, of course, at the sun, the hydrogen, which thus tinged it, is incapable of colouring anything, as its own light is absorbed by the transcendent brilliancy of the photosphere; while nothing would be more natural than to suppose that the light, which, in its own atmosphere, should strongly tinge anything radially illuminated, should be that of the prominences.

But the strongest proof of the variability of the outer portion and of the constancy of the inner portion is afforded by the observations made on board the small fleet attempting to save the *Pysche* off Aci Reale, where the eclipse was observed in unclouded splendour. Here were the ironclads *Lord Warden*, *Caledonia*, and *Royal Oak*, and the tugs *Weasel* and *Hearty*, besides the Italian gunboat *Plebiscito*, all within a stone's throw of each other. In all the drawings, and many have been received, we have a ring 5′ or thereabouts, while the outer portion is as variable as may be. On the same deck, that namely

of the flag-ship, *Lord Warden*, two drawings were made, one by Captain Brandreth, and the other by Dr. Macdonald, F.R.S., in which the variation is so strong that one would feel inclined to acquit the atmosphere of any participation in the matter, and to relegate the whole outer corona to subjectivity alone, did not Mr. Brothers's admirable photographs show both phenomena, as I am told they do. Dr. Macdonald saw eight rays arranged with perfect symmetry; Captain Brandreth saw only two elliptical hoops crossing each other at right angles.

Captain Cochran, of the *Caledonia*, besides the ring, saw a complicated stellate figure, the rays of nearly equal length, while Mr. Dexter, at sea between Catania and Syracuse, saw, besides the ring, *only one ray* of inordinate length.

So much for the drawings. I think that if the records of former eclipses be now examined, especially Mr. Carrington's drawing of the eclipse of 1851, and compared with the others taken at the same time, additional evidence will be gathered in favour of the compound nature of the corona, which, on the evidence now before me, I consider the great teaching of the present eclipse. Our experience in Sicily seems to be similar to that of the Spanish observers, for Mr. Perry writes that "some observed two curved rays," while the rapid degradation of light occurred at one-fifth of a solar diameter, but, so far as I know, no one in Sicily was favoured with a view of the dark intervals which were observed in Spain.

There is a strange and most interesting discordance between some of the spectroscopic observations made in Sicily and Spain. At Agosta, where the totality was well visible for ten seconds, Mr. Burton detected a green line near E, with a tangential slit (distance from moon not stated). This line, which was also seen by the Italian observers, is doubtless the one recorded last year by the American astronomers, but in Spain Mr. Perry states that bright lines at C near D, *b* (or E) and F were observed 8′ away from the sun. At Syracuse Prof. Harkness, whose telescope was moved into the various positions by Captain Tupman, R.M.A., found the green line in all parts of the corona, so far as about 10′ from the sun, and at one point thought he detected two green lines, less refrangible than it; but at several places he saw a complete hydrogen spectrum (including C), which he attributed to prominences, until he was informed by Captain Tupman that there was no prominence near the slit. More proofs of the terrestrial nature of this portion of the corona, I think, taken in connection with the fact that *the dark moon gave identically the same spectrum.* It would appear that there was so much atmospheric reflection in Spain, and here and there at Syracuse, that the true coronal spectrum with its line near E, the existence of which we must now accept as established beyond all question, was partially masked by the prominence spectrum with its usual well-known lines. There is one passage in Mr. Perry's interesting letter in which, if there be a misprint, as I suspect there is, an observation of great importance is recorded. It runs, "Mr. Abbay, observing at Xeres with a spectroscope of 2 prisms of 45° belonging to Professor Young, saw the bright lines C, D, F; and afterwards F and a line rather more bright than F on the less refrangible side of B, C not noticed then." Now, if *b* (not B) was intended here we have sub-incandescent hydrogen mixed with the green-line-giving-substance, which may probablybe a new element with a vapour density less than hydrogen.

So that roughly we might regard the chromosphere to be built up of the following layers, which are in the orders of vapour density in the case of known elements:—

X′ (new element)	. . . . .	Green coronal line
Hydrogen	Sub-incandescent	F
	Incandescent	C, F, near G, h
X (new element)	. . . . .	Near D
Magnesium	. . . . . .	*b* and lines in blue and violet
Sodium.	. . . .	D
Barium	. . . .	Several lines
Iron, &c.	. . . .	Several lines, including E

The foregoing table excludes naturally the substance or substances which give bright lines in the solar spectrum, which are at times visible in the spectrum of the chromosphere. I have ventured to suggest that the substance which gives the line in the green is a new element, because invariably I have found that in solar storms the chromospheric layers are thrown up in the order of vapour density, and because all the heavier vapours are at or below the level of the photosphere itself.

With regard to the question of polarisation, the parties in Sicily obtained evidence that the corona was radially polarised, though Professors Harkness and Eastman obtained a result which they explain differently. Mr. Ranyard, at Villamonda, and Mr. Peirce, jun., north of Catania, obtained identical results in favour of strong polarisation. Hence the solar corona, accepting these observations, not only radiates, but reflects solar light to us. A careful consideration of this fact, taken in connection with the possible addition of a, so to speak, terrestrial corona to its light, may enable us to account for some of the observations, both polariscopic and spectroscopic, which do not at first appear to harmonise with those to which I have referred, notably those which give a pure continuous spectrum to the corona, and which state that its light is only slightly polarised.

From what has preceded, then, we seem justified in suggesting as working hypotheses the following, which, however, more accurate information may alter, and which I offer as suggestions only, *bien entendu.*

1. The Solar Chromosphere extends some 5′ or 6′ from the sun (Watson and others), its last layers consisting of cool hydrogen (Mr. Abbay), and possibly a new element with a green line in its spectrum (Young, Burton, and others); which line, if it be identical with the auroral line as stated by Gould, may possibly be present in the higher regions of our own atmosphere.

2. Outside this stratum the rays, &c., are for the most part due partly to our own atmosphere, partly to our eyes, for their shape varies; they are seen by some at rest, by others in motion, and their spectrum is the same as that of the dark moon (Maclear).

3. The white light of the chromosphere above the prominences, as seen in an eclipse, is due to its strong reflection of solar light, as shown by the polariscopic observations (Ranyard, Peirce, jun., Ladd).

4. The rosy tinge of the corona proper, that is of the region more than 5′ or 6′ from the sun, is due to our atmosphere containing light which comes from both the higher and lower strata of the chromosphere (Peirce, sen., Maclear, Abbay).

Venice, Jan. 9 J. NORMAN LOCKYER

A HEARTH OF THE POLISHED STONE AGE

Note sur un Foyer de l'Age de la Pierre polie découvert au Camp de Chassey en Septembre, 1869. Par Ernest Perrault. 1870. 4to. Pp. 32, and 8 plates. (Chalon sur Saône, L. Landa. London: Williams and Norgate.)

ON the summit of a steep hill between the valley of the Bas Roches and that of the Dheune, overlooking the immense plain of the Saône and commanding a view of the Jura, the Alps, and the mountains of the Maconnais and the Morvan, and surrounded by numerous other camps, is the camp of Chassey, which occupies an area of about 800 yards in length by a breadth varying from about 100 to 200 yards. So commanding and important a spot was not only taken possession of by the Romans for a *castellum* and by the Gauls for an *oppidum*, but was also occupied in prehistoric times. Several collections of antiquities belonging to different periods have been formed upon the spot, but it was reserved for M. Perrault to make the interesting discovery which he has recorded in so simple yet so complete a manner in the pages now before us. A terrace, sheltered by rocks from the north and east winds and facing the morning sun, seemed to him well adapted for early habitations, while a depression in the ground in front proved, on examination, to contain the remains of a large hearth, or it might be termed kitchen, and here he instituted excavations.

Beneath a few inches of soil he found a bed rather more than two feet in thickness, made up of ashes, bones, and pottery, and containing numerous instruments of various kinds. The whole reposed on a platform of rough slabs of stone, blackened like the soil beneath them by the action of fire. Not a trace of metal was discovered, and in describing the objects found, M. Perrault divides them into (1) instruments of stone, (2) those of bone, and (3) pottery.

Exclusive of fragments some 150 stone instruments were found, consisting for the most part of hatchets, arrow-heads, flakes, borers, scrapers, hammers, mealing stones, and polishing stones. No less than eight perfect stone hatchets were found, as well as fourteen broken, and of those that were uninjured two were still mounted in stag's-horn sockets, similar to those with which the discoveries in the Swiss Lake dwellings have made us so well acquainted.

Only two are of flint, and one of fibrolite, the others being of chloromelanite, serpentine basalt, and diorite. They seem to have been formed from pebbles brought down by the Saône, and it is interesting to observe that the same process of manufacture was in use in this part of Burgundy as in Switzerland, the splitting of the pebbles into the required form having been partly effected by sawing. That some of the spare hours of those who frequented the hearth were employed in preparing their hatchets is proved by the large number of grinding or polishing stones, of which, counting fragments, upwards of sixty were present. M. Perrault regards one of the smallest of the cutting instruments, a little triangular celt, as a religious emblem, but it seems more probable that it was used as a hand-tool, like a chisel, of one of which the sharpened end was also found.

The arrow-heads of flint, twenty-three in number, present a variety of forms, leaf-shaped, triangular, lozenge-shaped, and tanged, the latter both with and without barbs. Their general aspect is such as might have been expected from the locality, most of the forms occurring also in Switzerland. There are, however, one or two shaped like small hatchets, with a broad sharp base, formed by the original edge of the flake from which they were made, and rounded or truncated at the other end. It is stated that this sharp edge was intended for insertion in the wood, but more probably it was the other end that was thus secured, and the arrows were, so to speak, chisel-pointed, like the flint-tipped arrows which survived in use, probably for fowling purposes, after metals became known to the ancient Egyptians. Similar arrow-heads, if such they be, have been found in considerable numbers in Sweden, and a few in Denmark, as well as in some other parts of France. It seems by no means impossible that some of the sharp-based instruments from the Yorkshire Wolds may have served a similar purpose.

The mealing stones consist of a large block, usually of hard sandstone or porphyry, and a smaller stone as muller, and are of the same character as those still in use in Central Africa. They must have been gradually eaten together with the flour they produced, and no doubt tended to promote that wearing away of the crown of the teeth so common in ancient times. None of the grain has been found, but probably most of the cereals known to the old Swiss Lake dwellers were also known at Chassey.

The objects in bone and horn are almost identical with those from the earlier Swiss Lake dwellings, and consist of the sockets already mentioned, awls, chisels, &c. The pottery, which is extremely fragmentary, is much of the same character as the Swiss. It has been ornamented both by punctured dots and by a sort of pillar moulding as well as by incised lines. In one instance there seems to have been an attempt to represent the outline of a boar by lines scratched in the clay when still moist. In another, the ornament consists of bands of triangles alternately cross-hatched and plain, a style more in accordance with the bronze age than with that of stone. Most of the pottery seems to have been adapted for suspension. The number of small ears or handles found exceeded 200. A few spindle-whorls and beads were also found, but the most curious objects are the spoons, exactly similar in form to those of wood in common use in our kitchens at the present day, but formed of clay. It is true that several wooden ladles and at least one earthenware spoon were found in the settlement of Robenhausen, but one can hardly repress a feeling of surprise at finding the spoon so fully and completely developed among a people apparently unacquainted with the use of metal, though it is true that they appear to have had the materials for porridge at their command.

In concluding this short notice of a valuable contribution to prehistoric archæology, a regret must be expressed that the animal remains discovered in the refuse heap have not, apparently, as yet been submitted to proper scientific examination, so as to determine the species, and which of

them were domesticated, though some human remains from neighbouring tumuli and interments are reported on by Dr. Pruner-Bey. The animals whose bones occurred are described as ox (possibly domesticated), pig, stag, sheep, goat, and horse, which is rare. The bones are not always broken, and the vertebræ occasionally occurred in juxtaposition, as if meat at times had been extremely abundant. There is no mention of any remains or traces of dogs, and this condition of the bones seems to afford an argument in favour of their absence, which, if established, would be a remarkable fact. Some teeth of reindeer are mentioned as having been found on the plateau, and it would be of great interest to ascertain their relation to the other remains. Let us trust that ere long there may again be a season in France when a though may fairly be bestowed on other camps and other earthworks than those on which attention is now so unfortunately concentrated. J. E.

SPONTANEOUS GENERATION

I HAVE repeatedly subjected various solutions for Dr. Bastian to a temperature of 150° to 156° C. in sealed vacuous tubes, in order that he might afterwards submit them to a microscopical search for living organisms. The result of this search led him to conclude that living organisms had been generated from non-organised matter, whilst Professor Huxley, who examined the contents of one of the tubes, considered that no such conclusion could be drawn from his own observations. I therefore determined to repeat these experiments, operating in exactly the same manner as before in the preparation of the solutions, the sealing them up in vacuous tubes, and exposing them to a high temperature, but taking additional and much more stringent precautions against the subsequent admission of atmospheric germs into the tubes.

For this purpose four tubes of hard Bohemian glass were prepared, and about half-filled with a liquid consisting of

Carbonate of Ammonia	15 grains.
Phosphate of Soda	5 grains.
Distilled Water	1 oz.

No care was taken to exclude living germs from these ingredients, reliance being placed, for the destruction of their vitality, upon the high temperature to which they were afterwards subjected.

These tubes were carefully exhausted by means of the Sprengel pump, and hermetically sealed; they were then, on July 18, 1870, exposed for four hours to a temperature varying from 155° to 160° C. in a Papin's digester. After being allowed to cool, the digester was opened, and the tubes immediately plunged, two of them into colourless concentrated oil of vitriol, and the remaining two into a nearly colourless saturated solution of carbolic acid in water. These precautions were taken in order to avoid the possible admission of atmospheric germs through invisible cracks in the glass; such cracks, entirely invisible to the eye, are known sometimes to exist, and to be in some cases so excessively minute as to require several days for the admission of enough air to perceptibly impair a torricellian vacuum within. By keeping the tubes entirely immersed in liquids which are immediately fatal to vitality, I hoped to meet any objections that might be raised, in the event of living organisms being subsequently found in the tubes, that the germs of such organisms had gained access to the enclosed liquids through invisible fissures in the glass. On examining them when they came out of the digester, it was evident that the interior walls of the glass tubes had been corroded by the enclosed liquid, and as the tubes had stood upright in the digester, it was easy to see, by the sharp limits of the erosion, the extent to which the liquid had expanded under the influence of the high temperature to which it had been exposed.

The cylinders containing the immersed tubes were now maintained at a temperature from 60° to 75° F., and were exposed to bright diffused daylight, and sometimes to sunlight, for a period of more than five months.

The liquid in all the tubes became more or less turbid, and in some cases a small quantity of a light flocculent precipitate subsided to the bottom. On the 24th of December last two of the tubes, which exhibited the greatest turbidity, were selected for examination (one of them had been immersed in concentrated sulphuric acid, the other in the solution of carbolic acid). The vacuum was unimpaired, and the liquid in the interior formed a good water hammer. These tubes were opened in the presence of Prof. Huxley and Mr. Busk, and we submitted their contents to a searching microscopical examination with powers varying from $\frac{1}{8}$th to $\frac{1}{16}$th. Especially was the flocculent sediment in the tubes subjected to careful inspection. So far as the optical appearances presented by the sediment go, they may be appropriately described in the terms which Dr. Bastian applied to the matter found by him in a solution of like composition and similarly treated (see NATURE, July 7, 1870, p. 200). "A number of little figure-of-eight particles, each of which was $\frac{1}{30000}$″ in diameter, were seen in active movement, even in situations where they could not have been influenced by currents. The portions of the pellicle were made up of large, irregular, and highly refractive protein-looking particles imbedded in a transparent jelly-like material. The particles were most varied in size and shape, being often variously branched and knobbed. There were also seen several very delicate, perfectly hyaline vesicles, about $\frac{1}{20000}$″ in diameter, these being altogether free from solid contents." But the movement of the particles which we observed was obviously mere Brownian motion; and many of the particles were evidently minute splinters of glass. There was not the slightest evidence of life in any of the particles. The water on the slide containing these solid matters was evaporated off, and they were treated with hot concentrated sulphuric acid, the temperature of the slide being raised to about 100° C. There was no blackening, and the rounded and dendritic bodies remained as entirely unaltered as the glass splinters. Indeed, some of the larger spheroidal bodies were evidently rounded particles of glass which had become detached from the inner walls of the tube by the corrosive action of the enclosed liquid at the high temperature to which it had been exposed in the digester.

London, January 16 E. FRANKLAND

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.*]

The Continuity of the Chalk

SIR CHARLES LYELL devotes a paragraph of his valuable "Students' Elements of Geology," just published, to the consideration of what he regards as a "popular error as to the geological continuity of the Cretaceous period." I feel the utmost diffidence in venturing to controvert any opinion of an authority so unrivalled in such questions, but as I believe the first definite suggestion of this view occurs in the report of the *Lightning* Expedition of 1868 by my friend and colleague, Dr. Carpenter, where it is specially associated with my name, I feel bound to defend so far as I can, or at all events to explain, an opinion which I then held on perhaps somewhat slender grounds, but which further investigation and reflection have since ripened into a firm belief. Sir Charles Lyell says (p. 263) that "certain points of resemblance which the deep-sea investigations have placed in a strong light, have been supposed by some naturalists to warrant a conclusion expressed in these words: 'We are still living in the Cretaceous epoch;' a doctrine which has led to much

popular delusion as to the bearing of the new facts on geological reasoning and classification." I do not say that the phrase "we are still living in the Cretaceous epoch" is defensible in a strictly scientific sense, chiefly because the terms "geological epoch" and "geological period" are thoroughly indefinite. We speak indifferently of the "Silurian period" and of the "Glacial period," without consideration of their totally unequal value, and of the "Tertiary period," and of the "Miocene period," although the one includes the other. It is ntended rather, I believe, in a popular sense, to meet what seems to be the general popular impression, that a geological period has, in the region where it has been studied and defined, something in the shape of a beginning and an end, that it is bounded by periods of change,—elevation, denudation, or some other evidences of the lapse of unrecorded time; and that it would be inadmissible to speak of two portions of the same continuous deposit, however distant the times of their deposition might be, and however distinct their imbedded faunæ, as belonging to two geological periods.

It was certainly under this idea that in an address to a popular audience in April 1869, I stated my belief that it is not only chalk which is being formed at present in the bed of the Atlantic, "but *the* chalk, the chalk of the Cretaceous period." Sir Charles Lyell says, in summing up his objections at the end of the paragraph, "the reader will at once perceive that the present Atlantic, Pacific, and Indian Oceans are geographical terms, which must be wholly without meaning when applied to the Eocene, and still more to the Cretaceous period; so that to talk of the chalk having been uninterruptedly forming in the Atlantic from the Cretaceous period to our own, is as inadmissible in a geographical as in a geological sense." I confess I do not understand the geographical difficulty; the "Atlantic Ocean" is doubtless a geographical term, but the depression under discussion occupies the area at present expressed by that term, and to use it seems to be the simplest way of indicating its position. That it is highly probable that the chalk has been so uninterruptedly forming over some parts of that area, is, however, exactly what I wish to show. I will therefore set aside the question of expression, and address myself simply to the consideration of the fact. And first with reference to the physical aspects of the case.

All the principal axes of elevation in the north of Europe and in North America have a date long anterior to the deposition of the Tertiary and even of the newer Secondary beds; and these strata were, consequently, all deposited with a certain relation in position to certain main features of contour, which are maintained to the present day. Many oscillations have, undoubtedly, taken place since, and every spot on the European plateau has probably many times alternated between sea and land, but it is difficult to show that these oscillations have occurred in the latitude of Britain to a greater extent than 1,500 feet up and down; a subsidence to that extent would, however, be sufficient to produce over most of the northern land a sea 100 fathoms deep, the average depth of the German Ocean.

From a glance at a geological map of Europe and North America, it would seem that the sum of these elevations and subsidences has produced a gradual elevation of the edges, and a general contraction, of a basin the long axis of which coincides roughly with the axis of the Atlantic. The Jurassic beds crop out along the outer edge of this basin, the Cretaceous beds form a middle band, while the Tertiaries occupy the troughs and valleys. All of these, however, maintain a certain parallelism, determined by the contour of the older mountain ridges, to one another and to the shores of the present sea.

From the parallel of 55° N. lat., at all events to the equator, we have on either side of the Atlantic a depression 600 to 700 miles in width, averaging 15,000 feet in depth.

These two valleys are separated by the modern volcanic plateau of the Azores. I cannot think it at all probable that any general oscillations have taken place in the northern hemisphere since the commencement of the Tertiary period sufficient to form that immense abyss, or, if formed, to convert it into dry land; but on this point I am able to quote the highest authority:—"If at any former period the climate of the globe was much warmer or colder than it is now, it would have a tendency to retain that higher or lower temperature for a succession of geological epochs. . . . The slowness of climatal change here alluded to, would arise from the great depth of the sea as compared to the height of the land, and the consequent lapse of time required to alter the position of continents and great oceanic basins. . . . The mean height of the land is only 1,000 feet, the depth of the sea 15,000 feet. The effect, therefore, of vertical movements equally 1,000 feet in both directions, upward and downward, is to cause a vast transposition of land and sea in those areas which are now continental, and adjoining to which there is much sea not exceeding 1,000 feet in depth. But movements of equal amount would have no tendency to produce a sensible alteration in the Atlantic or Pacific Oceans, or to cause the oceanic and continental areas to change places. Depressions of 1,000 feet would submerge large areas of the existing land, but fifteen times as much movement would be required to convert such land into an ocean of average depth, or to cause an ocean three miles deep to replace any one of the existing continents." (Lyell, "Principles of Geology," 1867, pp. 265-6.) The wide extent of Tertiaries in Europe and the north of Africa sufficiently proves that much land has been gained in Tertiary and post-Tertiary times, and the great mountain masses of Southern Europe give evidence of great local disturbance. But although the Alps and the Pyrenees are of sufficient magnitude to make a deep impression upon th senses of men, taking them together, their materials would, it spread out, only cover the surface of the North Atlantic to the depth of about six feet, and it would take at least 2,500 times as much to fill up its bed. It would seem by no means improbable that while the edges of what we may call the great Atlantic depression have been gradually raised, the central portions may have acquired an equivalent slight increase in depth; but it is most unlikely that while the main features of the contour of the northern hemisphere remained the same, an area of so vast extent should have been depressed by more than the height of Mont Blanc. On these physical grounds alone I should be inclined to believe that a considerable portion of this area has been continuously under water, and that consequently a deposit has been forming uninterruptedly from the period of the chalk to our own.

I would now refer to the palæontological bearings of the question. Sir Charles Lyell says (p. 263), "The reader should be reminded that in geology we have been in the habit of founding our great chronological divisions, not on foraminifera and sponges, nor even on echinoderms and corals, but on the remains of the most highly organised beings available to us, such as mollusca. . . . In dealing with the mollusca, it is those of the highest or most specialised organisation which afford us the best characters in proportion as their vertical range is the most limited. Thus the cephalopoda are the most valuable, as having a more restricted range in time than the gasteropoda, and these again are more characteristic of the particular stratigraphical sub-divisions than are the lamellibranchiate bivalves, while these last again are more serviceable in classification than the brachiopoda, a still lower class of shell-fish, which are the most enduring of all." With great deference to Sir Charles Lyell, I cannot regard the most highly specialised animal groups as those most fitted to gauge the limits of great chronological divisions, though I admit their infinite value in determining the minor sub-divisions.

The culmination of such animal groups, such as we find in the marvellous abundance and variety of both orders of cephalopods at the end of the Jurassic and the commencement of the Cretaceous period, undoubtedly brings into high relief, and admirably illustrates to the student, the broad distinctive characters of the Mesozoic fauna; but speaking very generally, the more highly a mollusc is specialised, the shallower is the water which it inhabits. The cephalopods are chiefly pelagic and surface things, and their remains are consequently found in deposits from all depths. The gasteropods, with comparatively few exceptions, range from the shore to 1 to 200 fathoms, and lamellibranchs become scarce at a slightly greater depth; while some orders of crustacea, brachiopods, echinoderms, sponges and foraminifera, descend in scarcely diminished numbers to a depth of 10,000 feet. In fact, the bathymetrical range of the various groups in modern seas corresponds remarkably with their vertical range in ancient strata.

A change in the distribution of sea and land involving a mere change in the course of an ocean current, might modify the conditions of an area for all cephalopods, pteropods, heteropods, and other surface living animals of high type, even to their extinction. By oscillations of 500 feet up and down, the great mass of gasteropods and all reef-building corals, would be forced to emigrate, become modified, or destroyed, and another hundred fathoms would exterminate the greater number of bivalves; while elevations and depressions to ten times that amount might only slightly affect the region of brachiopods, echinoderms, and sponges.

In the late deep-sea dredgings by M. de Pourtales, off the American coast, and by H.M.S. *Lightning* and *Porcupine*, and

Dr. Marshall Hall's yacht *Norna* off the west coast of Europe, no animal forms have been discovered, so far as I am aware, identical with chalk fossils. Additional evidence has, however, been procured that over a large part of the bottom of the Atlantic a deposit is being formed mainly of disintegrated globigerinæ and other foraminifera and coccoliths, which appears to be undistinguishable from the ancient chalk.

Not fewer than twenty genera of vitreous sponges have been dredged belonging to two groups, the Hexactinellidæ and the Lithistidæ of Oscar Schmidt, both of which groups are highly characteristic of the chalk and greensand. These sponges are in vast numbers, like the ventriculites and their allies in older Cretaceous beds; and they, with other silicious organisms equally abundant in the modern chalk area, seem to be capable of supplying that amount of silica in a fine state of division which might explain the production of chalk flints. A large series of echinoderms were found, recalling to a remarkable degree, from the profusion of Cidarids and of star-fishes of such genera as Archaster, Astrogonium, and Stellaster, the general facies of the chalk echinoderm fauna; and besides this general resemblence, members of several families have been recovered which were supposed to be extinct. *Salenocidaris varispina* A. Ag., dredged by De Pourtales in the Strait of Florida, is a living Salenia; *Echinolampas caratomoides* A. Ag. perpetuates some of the most marked characters of the Galeritidæ. *Pourtalesia*, a genus first found by Count Pourtales and afterwards in the *Porcupine* expedition, is a true Dysaster. *Porocidaris purpuratus*, a fine species dredged off the Butt of the Lews, represents a genus hitherto known only by some isolated plates and radioles. Two very remarkable generic forms, dredged from the *Porcupine* off the coasts of Scotland and Portugal, only known from some fragments in the English white chalk, found a new family near the Diademidæ. Some new ophiurids approach their fossil ancestors; and off the coast of Portugal the dredge brought up at one rich haul twenty or thirty examples of a fine *Pentacrinus*; while over the whole area *Rhizocrinus loffotensis*, a degenerate little Bourgueticrinus, seemingly one of the last of the pear-encrinites, is abundant.

I am not in a position to say much about those groups which I have not personally examined, except that Mr. Gwyn Jeffreys and Prof. Martin Duncan report that among the mollusca and corals many species occur which have been hitherto known only as fossils, principally, as might have been expected, in comparatively shallow water forms in the Tertiaries.

I do not see that there is any object in attempting to explain this singular resemblance between these deep-sea deposits in the Atlantic and the old chalk in composition and structure and in embedded fauna on any other assumption than that of a continuity of conditions over some part at all events of the area. During the lapse of time, while the fauna of shallower water has again and again undergone almost total change by changes in the distribution of temperature and in the distribution of sea and land, the fauna of the deep water has been also affected. To a depth of 5,000 feet it is at present heated over a large portion of the North Atlantic many degrees above its normal temperature. Accepting, as I believe we are now bound to do, some form of the doctrine of the gradual alteration of species through natural causes, one is quite prepared to expect a total absence of the identical forms found in the old chalk. The utmost which might be anticipated is such a resemblance between the two faunæ as might justify the opinion that the later fauna bears to the earlier the relation of descent with extreme modification. Sir Charles Lyell asks if we have dredged belemnites, ammonites, baculites, hamites, turrilites, &c.; that question is, I think, best answered by the record of the old Cretaceous beds themselves, which are scarcely more remarkable for the presence of these singular and beautiful forms than for their rapid extinction. According to the view which I have felt myself compelled to adopt, the various groups of fossils characterising the Tertiary beds of Europe and North America represent the constantly altering fauna of the shallower portions of an ocean whose depths are still occupied by a deposit which has been accumulating continuously from the period of the pre-Tertiary chalk, and which perpetuates with much modification the pre-Tertiary chalk fauna. I do not see how this view militates in the least against the "reasoning and classification" of that geology which we have learned from Sir Charles Lyell. Our dredgings only show that these abysses of the ocean which Sir Charles Lyell admits in the passage quoted above to have outlasted on account of their depth a succession of geological epochs, are inhabited by a special deep-sea fauna possibly as persistent in its general features as are the abysses themselves.

WYVILLE THOMSON

Ocean Currents

ATTENTION has been much drawn of late to the subject of Ocean Currents and their causes, and it has occurred to me that there is a directing if not an originating cause of these streams, which has, so far as I am aware, been overlooked by physicists. It is known* that at some parts of the earth's surface there exists an atmospheric pressure capable of sustaining a column of mercury in the barometer of upwards of 30 inches in height; at the same time there are certain areas over which this pressure is only such as to raise the barometric column to a little over 29 inches. Now if we compare the difference of absolute weight sustained by two such areas, we shall see that in the space over which the higher atmospheric pressure exists, there is an excess of weight of air, amounting in round numbers to 1,000,000 of tons on each square mile. Applying this fact to the region of the ocean in which the surface currents are best known, the North Atlantic, we find from the isobaric chart that there is throughout the year over a large portion of the eastern side of this sea, next the coast of North Africa, a pressure (to use the convenient mode of expressing it) of upwards of 30·2 inches. To westward of this space, towards the Gulf of Mexico and the coast of the United States, the average pressure decreases; between Newfoundland and the British Isles the pressure is still diminished, till in the wide channel between Iceland, Norway, and Spitzbergen, we arrive at a yearly pressure of less than 29·6 inches. It is reasonable to believe that the waters which lie under the high pressure area have a tendency to escape from under the excessive weight, towards the space over which the pressure is less. But the high pressure area next the African coast is precisely that upon which the north-east trade winds descend, and the waters, aided in their choice of an exit, will naturally flow off to south-westward before the wind. Their continuance in this direction is barred, however; for across the whole of the southward passage between Africa and South America, there exists another belt of high pressure, out of which the south-easterly trade winds blow. The only course left for the escaping waters (allowing for the moment that the excess of pressure *is* a cause of their movement) is to westward, where the pressure is lessened, towards the Gulf of Mexico, and the east coast of America, and thence towards the low pressure space between Iceland and Norway. But this is exactly the course that the Gulf Stream, or rather the North Atlantic warm stream of which the Gulf Stream is the most prominent feature, is seen to take. Are we not then warranted in concluding that the difference of atmospheric pressure has some power both in originating and in directing the course of this ocean current?

In suggesting the unequal distribution of atmospheric pressure as a supplementary cause to difference of temperature and of density, to evaporation, rain, and winds, and to whatever further agents there may be in the production of ocean currents, I would venture to express a hope that some one in authority, by carefully comparing and valuing the power of each one of these motive forces, and their application to the known streams, will give to the world a system of the causes of ocean currents which will be vastly more relevant to the phenomena these streams are known to present, than any one of the theories which have as yet been put forth.

When we know that Sir John Herschel gives to the winds the entire right of setting the ocean streams in motion; that Captain Maury holds the universal circulation of the sea to be caused by nothing else than the differences in its specific gravity, and that Dr. Carpenter (or rather Professor Buff) would bring about a general interchange of polar and equatorial water by the aid of sunshine and frost alone; is it not time to ask which of these three causes we should accept as the true one, or if all three are partially concerned, what part is to be taken from each to let the others have their fair share in the work?

KEITH JOHNSTON, Jun.

The Measurement of Mass

I AM happy to meet with an opponent who comes so directly to the point as my Reviewer, W. M. W.

* I would refer those who desire to look more particularly into this matter to the monthly isobaric charts prepared by Mr. Buchan, to illustrate his admirable paper on the mean pressure of the atmosphere. Trans. Royal Soc. Edin., vol. xxv.

The gist of his argument lies in his assertion, that "if a true pound, as determined at London, were carried to the North Pole, it would weigh more than a pound."

Now, since the determination of a pound is actually effected by making it a copy of a standard so that they shall counterpoise each other *in vacuo*, it is strictly independent of locality.

The standards actually employed for this purpose, for example, the authorised copies of the principal standard pound which have been sent to various countries, have been made as nearly identical in mass as skilled workmen could make them, and have been sent at random to different latitudes.

Similar remarks apply to the weights in a chemist's box. Surely it is not seriously proposed that the chemist should file them down to accommodate them to the increase of gravity, when he takes them from London to Edinburgh.

Where should we find ourselves, if the makers of chemists' weights endeavoured to make them of different masses, according to the places where they were to be used?

Fortunately, this has never been attempted; and since all parts of the world are in possession of practically identical standards of mass, under the name of standard pound or standard kilogramme, and tolerably accurate copies of these and their fractional parts are in everybody's hands, why not acknowledge them as standards of mass, which they are in point of fact, although, according to the theory which I am combating, they ought not to be?

Those who hold that theory must choose between two evils:—they must either make the pound a unit of force, in which case they must file or load their weights as they go from place to place (this seems to be the alternative which W. M. W. chooses); or they must accept the gravitating forces of equal pieces of metal as nominal units of force at the different localities where these pieces of metal may happen to be, although these forces are really not equal. This latter alternative, which gives a variable unit of force, has been commonly adopted till recently, and a variable unit of mass has been conjured up to suit it.

If our spring-balances were as accurate as our standard weights, W. M. W.'s idea would be practicable. The equality of two forces at different places could then be very directly tested; but, in fact, the most accurate means we possess of making such a comparison consists in a double process, a weighing with the ordinary balance, combined with a difficult and less accurate pendulum-comparison. Inasmuch then as ease and accuracy of comparison is the first essential of a scientific standard, I submit that the world is right in employing standard pounds of equal mass and not standard pounds of equal gravitating force.

Much confusion arises from using the word *weight* in a connection which leaves it doubtful whether *mass* or *gravitating force* is meant. I trust you will keep your columns open for the further discussion of this question, as it much needs ventilation.

Belfast, January 11 J. D. EVERETT

The Tails of Comets, the Solar Corona, and the Aurora considered as Electric Phenomena

IN reference to a letter from Mr. Bedford, Ph.D., published in your last number, allow me to state—

1. That I had never seen or heard of Dr. Bedford's theories.
2. That, judging from the extract given in his letter, Dr. Bedford has not published anything analogous to the electrical hypothesis which I have put forward. In his letter he has misquoted and made omissions to the extent of one-half the heading, in order, I suppose, to avoid the very mention of the word electricity, which, on the other hand, is the very substance of my postulate. OSBORNE REYNOLDS

Owens College, Jan. 17

Apparent Size of the Moon

FROM almost any place in the balcony of St. James's Hall, at an evening performance, one may find in the coronets of gas-jets, forming one or more of the arches across the roof, corroboration of what I wrote in NATURE of May 12, 1870 (vol. ii. p. 27). The nearest or first coronet, and the two next, observe a gradation of increase in apparent magnitude, as they should do. But the rest, which (unlike those three) descend towards the horizon, and should, nevertheless, observe a gradation of increase, are apparently all of a size. I call attention to this "unconsidered trifle" as having the full force of a very elaborate experiment. C. M. INGLEBY

Ilford, Jan. 7.

Atmospheric Currents

THE following is part of a letter signed "G." in NATURE of 6th October, 1870:—

"It is very important to obtain correct and copious data regarding the atmospheric currents between say 5,000 feet and five miles above the level of the sea, and especially at various points on and near the equator, and at about 30° to 32° North and South latitudes. Within these limits the rain-bearing currents of the atmosphere move. If self-registering meteorological instruments were placed permanently upon several of the leading mountain ranges of the world, and their records copied at stated intervals, we should obtain valuable data for determining the direction, velocity, and magnitude of the controlling atmospheric currents of the globe."

This suggestion is most valuable, but it would involve a source of error that would be difficult to allow for. Most mountain ranges occupy so extensive an area that they have their own local climates, and indications on such ranges would consequently not give accurate information about the currents where they are not modified by such influences. The most valuable information will be obtained from the most isolated mountains. The most isolated mountains of sufficient height, within sufficiently easy reach of us, are Etna and Teneriffe. Etna is in the variables, and Teneriffe in the trade-winds.

I do not, however, agree with the following suggestion, that more valuable observations still might be obtained by means of captive balloons. Balloons cannot be used in stormy weather, and they are also subject to the great disadvantage, that they tell nothing about barometric fluctuations: because the height of a balloon above the earth can be known only from the barometer, and consequently there is no way of ascertaining the indications of the barometer at a known height. It is not at all certain that the barometric curves at the summit and at the base of Etna or Teneriffe would present any close correspondence. Sets of barometrical and other meteorological observations taken for sufficiently long periods at the summit and at the base of such mountains would be probably the most valuable of all data for meteorological science in its present state.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, Jan. 14

THE AMERICAN ECLIPSE EXPEDITION

THAT portion of the American Eclipse Expedition which was stationed at Xeres was favoured by weather which enabled the observers to examine the Corona during the whole period of totality, and with some results of interest; an informal account of a part of which the kindness of Professor Winlock enables me to lay before your readers.

The party was in charge of Professor J. Winlock, and stationed in an olive grove about a mile from the city, the property of Mr. Davies, of Xeres, to whom all the members of the expedition are indebted for continual aid and kindness. The observations for the determination of longitude, latitude, and times of contact were conducted by Assistant G. W. Dean, of the Coast Survey, aided by Captain Ernst, of the U.S. Engineers, and by Mr. Gannett, of the Harvard College Observatory.

Their labours were facilitated by the courtesy of the Director of the neighbouring Observatory of San Fernando, with which telegraph connection was established, and the results attained will be found in full in their official report.

Besides the instruments of precision, the party was provided with four equatorial telescopes of from six to eight inches aperture, driven by clockwork, and many smaller ones. One of the largest telescopes carried an objective specially corrected for actinic rays, with which, and a companion instrument, photographs were taken during the totality and previous stages by Mr. Willard and his assistant.

In addition, a photographic telescope of upwards of thirty feet focus, placed horizontally, and receiving the solar rays from a heliostat, was used, in charge of Mr. Gannett.

The accessories of the instruments were such as the previous experience of the observers (nearly all of whom had witnessed the eclipse of August 1869) suggested, and were too numerous for description here, though a means

of rapidly recording the position of the lines noted in the Spectroscope, devised by Professor Winlock, proved of too essential service to be left unnoticed. Each spectroscope was provided before leaving America with this attachment, which enabled the observer to record the position of the lines as fast as he could point on them, and with a precision that compared favourably with micrometrical measurement.

I pass to the work done by each observer.

The Spectroscope used by Prof. Young was the one recently described in your pages; and through this he was enabled to watch the occultation of the protuberances of the photosphere, and announce the approach of the moon, some fifteen seconds before "first contact." At this time the sky about the sun was nearly clear, and numerous successive photographs were taken, with the accompanying chronographic record of the instant of exposure. Heavy clouds then obscured the sun, at intervals, till totality was very near, when a happy fortune gave us a few minutes of almost clear sky while it lasted.

It is important to explain that, though the sky was so little obscured that the Corona was visible during the whole of totality, there was during most of this time a very slight haze. I recall a short view of sky, distinctly blue in spite of the darkness; and the structure of the protuberances was continuously visible through the telescope; nevertheless, the presence of this partial haze should be remembered, as having a possible bearing on some of the conclusions to be drawn from what follows.

I give as nearly as I briefly can the principal results of each observer, as regards the corona only.

Professor J. Winlock, using a spectroscope of two prisms on a five-and-a-half-inch achromatic (directed by Mr. A. Clark as the finder) found a faint continuous spectrum, without dark lines.

Of the bright lines, the most conspicuous was 1474 Kirchhoff, which was followed all around the sun to at least 20′ from the disc. It may be here remarked that all the spectroscopes showed this as much the most conspicuous coronal line. A number of other lines were also noted, and their position recorded by the apparatus just referred to.

Professor Young's observations of this line were similar; he estimates its least extension at one half the solar diameter. With the slit of his spectroscope placed tangentially at the moment of obscuration, and for one or two seconds later, the field of the instrument was *filled with bright lines*. As far as could be judged, during this brief interval every non-atmospheric line of the visible spectrum showed bright; an interesting observation, confirmed by Mr. Pye, a young gentleman whose voluntary aid proved of much service. The observation of Mr. Pye was made with a spectroscope of one prism, before which was placed a small telescope arranged at Prof. Young's suggestion, not to give, as usual, a definite image on the slit, but to supply light from all portions of the corona and neighbouring sky with more intensity than would be furnished by directing the instrument toward the sun without it. From the concurrence of these quite independent observations, we seem to be justified in assuming the probable existence of an envelope surrounding the Photosphere, and beneath the Chromosphere usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all, or nearly all, the ordinary lines, showing them, that is to say, *bright* on a dark field.

Mr. Abbay, of Wadham College, Oxford, assisted with a Spectroscope prepared by Prof. Young for collateral observations to his own, and obtained results which have already been made public.

The Polariscope was used by Prof. Pickering at a station half a mile distant from the rest of the party. Using successively an Arago Polariscope, one of the form employed by Prazmowski, and a Savart, he is understood to have obtained with all three, results pointing to a radial polarisation of the Corona. The light covering the moon's disc he observed to be polarised throughout in the same plane, and the observations showed that the Arago and other Polariscopes dependent on colour were sufficiently delicate to determine this plane with accuracy.

At the same time Mr. Ross, his assistant, using the instrument employed by Prof. Pickering in the last eclipse, obtained the same results as were then found. Mr. Ross used a modification of the Bunsen photometer, and obtained several concordant measurements, showing that the light was equal to that of a standard candle at two feet.

The writer used a Savart's polariscope attached to a small telescope of $1\frac{1}{4}$ inches aperture, and having a field of about 2°.

The observations with the Savart's polariscope being subject to ready misconception, the preparation for observation and the appearance during it are here given with some minuteness.

Before the eclipse the Savart was so adjusted that the bands were most distinct when vertical, viewing the meridian sun reflected from water. None were visible when the sun was directly scrutinised before or after totality.

During totality the appearance which presented itself was unexpected.

The bands were distinctly seen on the corona, and were brightest where normal to the limb and where tangential to it. As the polariscope was slowly rotated, no marked diminution of their brightness was seen, and when it had been turned through 45° they were as bright as before; distinctly visible even in colour, and they so remained, the rotation being continued for greater security through a whole revolution, during the whole time they presented the appearance described, and characteristic of radial polarisation. They were not noticed on the disc of the moon, but this may well have been from the observer's attention being so exclusively directed to verifying their persistency on the corona.

The writer also employed a good achromatic, of four inches aperture, with a power of about 150, in the direct study of the coronal structure with negative results.

On the closest scrutiny of the part nearest the sun, nothing was seen but a nearly uniform diffuse light, except that one "dark ray" in the field was noticed to be absolutely straight and nearly radial. The outline of the Corona was roughly quadrangular, and a heavy field bar provided for the purpose being carefully set during totality in the direction of the longer diagonal, was found on subsequent estimation to make an angle of as nearly as possible 45° with the vertical.

The "red flames" were beautiful objects, as incidentally noticed during the telescopic scrutiny of the Corona, but not, I may add, more distinctly or more in detail than I had viewed them the day before through the Spectroscope of Professor Young.

The coronal structure is well shown on a photograph obtained by Mr. Willard during totality; a very interesting drawing of it was also taken by Mr. Gordon, a gentleman resident in the vicinity; and all the observers have descriptions to give of its appearance to the naked eye, which differ in some degree from each other. If I compare my impressions with those of others, or even with my own of last year, I find difference enough to suggest the probability of considerable "personality" in all such statements. In some well-marked features all agree, in other minor ones such difference exists that one might almost say each saw a different Corona. The observations with the Spectroscope and Polariscope are happily more removed from uncertainty.

The conclusions reached with both are not perhaps as absolute as they would have been with the cloudless sky of last year's Eclipse, they are still such as to fully justify, we may hope, the cost of labour and time in obtaining them, and which none of those present can regret.

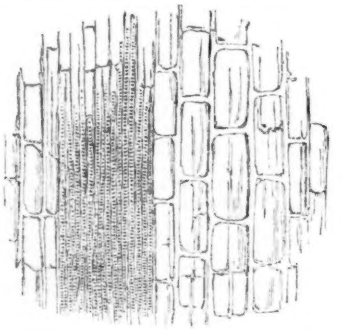
S. P. LANGLEY

SUGAR

II.

THE Beet-root (*Beta vulgaris*) is a native of the south of Europe, and is said to have been introduced into England in 1656. It is a hardy biennial plant with reddish purple leaves and large fleshy roots, which in some varieties are long or spindle-shaped like a carrot, in others short and thick, almost like a turnip. The colour also varies, some forms being of a deep purple, while others are of a dirty white with a purple tinge. Internally the beet-root is of a blood-red colour. It is well known with us both as a pickle and as a salad plant, but it is much more extensively used even for these purposes in Germany and France than it is in England.

Numerous varieties of the beet are in cultivation, but one known on the Continent by the name of Betterave à Sucre is extensively grown as a sugar-producing plant; and the trade in beet-root sugar is a very important one throughout France, Belgium, Germany, and Russia. The bulk of the sugar consumed in these countries is furnished by this plant. Its cultivation for the produce of sugar has been more than once attempted in this country, but hitherto on too small a scale to be successful. Latterly, however, more attention has been paid to it, and more spirit and energy shown by those who have taken up the question towards overcoming obstacles that were at one time considered insurmountable; but whether the results will continue to prove remunerative remains to be seen. So long ago as 1837 a refinery solely for the manufacture of beet-root sugar was established at Chelsea; and many acres of land

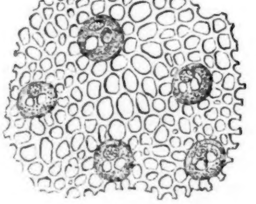


LONGITUDINAL SECTION OF SUGAR-CANE, MAGNIFIED

in the neighbourhood of London were devoted to the cultivation of beet. The discovery of the presence of sugar in beet was made in 1747 by a Prussian chemist named Margraaf, but his discovery was at first considered rather in the light of a scientific than of a practically useful character. It formed the subject of a communication to one of the learned societies in Berlin; and for several years afterwards the sugar so produced was considered an article of curiosity, and was consequently sold at fancy prices. Forty or fifty years elapsed before any experiments were made for the purpose of putting the discovery into practice, with the idea of extracting the sugar for actual use; these experiments however did not succeed, owing to the imperfect manner in which they were carried out; and the beet sugar manufacture would possibly even then have resulted in utter failure, had not Napoleon I., by excluding British Colonial produce from France, rendered it necessary for some new method to be devised for supplying France with sugar. Prizes were offered and many plans submitted. The Government, however, gave its support to the beet as the most likely source of success. Experiments were again renewed, and the result proved satisfactory, so that by 1812 this branch of manufacture was, for a time, placed on a firm footing in France. In 1814, however, the French markets again being opened, large quantities of cane sugar at once appeared from the West Indies, and beet sugar again fell into the background. A system of heavy and increasing duties continued to be levied upon colonial sugar till the year 1822, when the duty became so high as actually to amount to a prohibition to its entrance into French ports, and again the beet-root factories began to flourish. At the present time, or rather before the War broke out, hundreds of millions of pounds were made and consumed. Germany and France produce the largest quantity, Russia following close behind. It is, however, feared that the 300,000 tons of beet-root sugar, which was the estimated produce of France for 1870, will have been for the most part lost, owing to the interruption of the harvesting of the roots, and the consequent stoppage of the operations of the sugar factories.

Like the sugar-cane, the beet varies in quality and in the quantity of saccharine juice, according to the climate, soil, and mode of culture.

After the roots are dug up they are cleaned, usually by scraping them with a knife; they are then either sliced or rasped, and reduced to a pulp, which is placed in canvas bags and submitted to high pressure, by which means the juice is expressed. The pulp undergoes a second and sometimes a third squeezing, so as to obtain all the saccharine matter. This juice or liquor is then heated in a copper, and filtered and boiled with lime and water. A scum rises to the surface, which is taken off, and the juice is again boiled till it becomes of a proper consistency, when it is crystallised in a similar manner to cane



TRANSVERSE SECTION OF SUGAR-CANE, MAGNIFIED

sugar. Beet-root juice as expressed is very clammy to the touch, is nearly colourless, has a strong disagreeable smell, and contains a larger quantity of nitrogenous matter than cane juice. It is capable of being clarified and refined, so that it is made almost, if not quite, equal in appearance to the most superior descriptions of lump or moist sugar prepared from the sugar-cane.

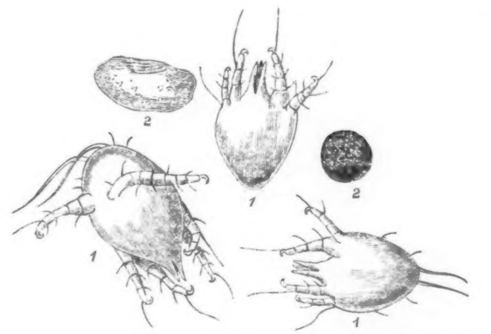
"Beet-root sugar is not only identical with cane sugar, but much of the Dutch lump sugar is actually the produce of beet-root. The circumstance cannot be too much insisted upon, that the seeming distinction between yellow beet sugar and yellow cane sugar depends on the extraneous coloured matters present. These, when eliminated by refining, leave white materials in all respects identical. There is positively no difference between these two, whether of colour or of grain. Grain or crystals can be developed from either to the size of the largest candy if desired; in fact, large white crystals produced from beet-root are sent in quantities from France into this country to compete with London, Bristol, Scotch, and other crystal manufactories."

As beet sugar has become of late in more general use in this country, so has cane sugar found its way in large quantities on the Continent. The mixing of these sugars can scarcely be considered in the light of adulteration, except when an inferior kind of one is mixed with a superior quality of another. The perfect system of filtering adopted in the manufacture of beet-root sugar causes it to be much freer from extraneous matters than

cane sugar, small pieces of the cane itself frequently occur in some descriptions of the latter. Of course it is impossible for them to be contained in loaf sugar, sugar-candy, or the finer kinds of sugar which undergo a careful process of purification.

A great deal has been said about the adulteration of sugar with sand, powdered marble, bone-dust, &c., and these tales are readily believed in by many people; but it is very unlikely that such insoluble substances would be used, as they would be sure to be detected. We know that inorganic substances are present in sugar, but we are inclined to believe that they are traceable to the imperfect cleaning of the cane or to the accidents, if we may so call them, attendant upon the process of manufacture.

Amongst the organic impurities often to be found in sugar, besides fragments of the cane before alluded to, are glucose or grape sugar, vegetable albumen, starch, minute fungi, and very frequently a number of small insects known as the sugar beetle (*Acarus sacchari*). The first of these, grape sugar, we have before described as being distinct from cane sugar, though its composition is nearly similar; as it is found most abundantly in grapes, whence its name; but it is also contained in various other fruits, as well in the dried state as in the fresh; and, moreover, it can be produced by the action of dilute sulphuric or other mineral acids, on starch or woody fibre, and so, in short, can be manufactured, which cane sugar cannot. It does not so readily crystallise as cane sugar, and the crystals are irregular, or have no



MICROSCOPICAL APPEARANCE OF THE SUGAR INSECT (*Acarus sacchari*).—1. PERFECT INSECT.—2. OVA

definite form. It is much more clammy in an ordinary way, and dissolves or runs to juice at the least possible moisture of the atmosphere to which it is exposed. It is of less sweetening power than cane sugar, and is much more liable to ferment, therefore its presence with cane sugar tends to lessen the value of the latter.

The purest form of cane sugar to be obtained is that known as "lump," and for this reason it is always used in preserving fruit, as the chances of fermentation are thus diminished. Sugar-candy and all the large crystal or dry sugars are pretty sure to be free from glucose, because its presence would, according to the proportions, cause the sugar to be more or less clammy or moist. The presence of vegetable albumen and starch in cane-sugar is one step towards its conversion into grape sugar or glucose, and consequently increases its inclination to ferment. The removal of this vegetable albumen is one of the objects, as well as one of the difficulties, of perfect sugar refining.

The presence of minute fungi in sugar can only be detected by the aid of a microscope, but the sugar-beetle may be seen with an ordinary lens.

We know of no better way of detecting it along with other impurities than one which has been before recommended, namely, to place a teaspoonful or two of sugar in a glass of tepid water, let it stand till the sugar has completely dissolved, when the insects will be found, some perhaps at the top of the water and others at the bottom amongst the sediment. It is supposed that this insect is the cause of the irritation of the skin, more especially of the hands, from which grocers and those who have the handling of sugar are said to suffer.

Many other plants besides the sugar-cane and beet are used in their respective countries as sugar-producers. Thus, while we and our European neighbours receive our supplies from the two plants just alluded to, the chief sugar-producer in North America is a species of maple (*Acer saccharinum*), a large forest-tree; and in the East Indies several species of palms yield sugar.

With regard to the effects of sugar upon the system, it is abundantly proved that it is most valuable. Nature has so provided that it shall be diffused not only in the sap of young and growing plants, but also in milk, the food of the young of all Mammalia. Though sugar may be considered as a force producer, it is much more adapted to the young than to the adult, for it is not capable by itself of producing muscle.

JOHN R. JACKSON

NOTES

IN addition to the details given this week of the Eclipse Expedition by Mr. Lockyer and Mr. Langley, we have received from Prof. W. G. Adams a report of the Agosta section of the Expedition, publication of which we are compelled to defer till next week.

IT is with great pleasure we hear of the appointment of the Rev. F. W. Farrar to the Headmastership of Marlborough College. This institution has already set an example to our other public schools in the cultivation of Natural Science, which will, we trust, receive a fresh impulse under the new headmastership.

WE rejoice to hear that there is at last a chance of a large amount of scientific knowledge being required at the hands of naval and military officers. This is as it should be, and we earnestly hope that the long-needed reform will not stop here.

THE Austrian astronomers, MM. Weiss and Oppolzer, who went to Africa to observe the Eclipse, were, we regret to learn, as unfortunate as the other observers on that continent.

AT a meeting of the Master and Senior Fellows of Caius College, held December 14, 1870, the following College order was passed:—"That until as many as forty shares of the College income are applied to the promotion of the Natural Sciences and Experimental Physics, a sum equal to the deficit (estimated at 20*l.* a share) be taken from the balance for distribution, and paid to a fund called the 'Natural Science Fund.' That the shares applied to any Fellowship or Scholarship awarded for proficiency in the Natural Sciences and Experimental Physics be counted as so applied. That the Natural Science Fund be applied solely to the promotion of the study of Natural Sciences and Experimental Physics in any way whatever which, in the judgment of the College, may seem proper."

THE Birmingham and Midland Institute has done equal honour to Professor Huxley and to itself, by electing him to the office of President, in succession to the late Charles Dickens.

DR. WOOD has been appointed Lecturer on Chemistry at the St. Mary's Hospital Medical School, in succession to Dr. Russell, transferred to St. Bartholomew's.

WE understand that Dr. Michael Foster and Mr. Watts are engaged in translating Kühne's "Physiological Chemistry."

WE hope in succeeding numbers to give in full Dr. Carpenter's and Mr. Gwyn Jeffreys's Report on Deep-sea Researches carried on during the months of July, August, and September 1870, in the *Porcupine*, recently read before the Royal Society.

WE quote the following pleasant "international" from the *New York Technologist*:—In view of recent remarks concerning the action of the British Government in regard to the expedition proposed by British scientific men, for the purpose of observing the approaching eclipse, it is but justice to say that on proper representation being made to Mr. Lowe, the Chancellor of the Exchequer at once expressed his opinion that such an expedition was one eminently worthy of government aid, and perfect arrangements as regards ships, funds, &c., have been made. One good result has, however, attended this delay on the part of the British Government—it has afforded an opportunity for the exchange of very pleasant courtesies between the scientific men of the United States and those of Great Britain. As soon as Prof. Peirce heard that their government had refused the use of the necessary ships, he at once tendered to the English scientists all the facilities at his command; and they were ample. No one doubts that, under similar but opposite circumstances, the same offer would have come from the other side; for, however stubbornly *Alabama* controversies may hold out, science takes no cognizance of them."

WE are informed that the series of penny lectures on Natural Science, delivered at the Hulme Town Hall, are to be obtained in London of Mr. Pitman, Paternoster Row.

A DEPUTATION from University College, London, consisting of Mr. George Grote, the president, the Hon. George Denman, M.P., Mr. Julian Goldsmid, M.P., and Dr. Storrar, members of the Council; Prof. Fuller, C.E., Prof. Williamson, F.R.S., and Mr. J. Robson, the secretary, has waited upon the Duke of Argyll, at the India Office, to present a memorial from the Council and the Senate of the College, on the subject of the proposed institution of a new Engineering College for the Indian Service. The interview with his Grace lasted upwards of an hour. The memorialists consider that the deficiency in the present system has arisen, not from any defect in the existing places of education, but from the injudicious system of examinations hitherto pursued, and from a want of sufficient inducements to well-qualified men to enter on the career proposed to them. The memorialists assume that the latter cause is recognised by the Government, for it is understood that it is in contemplation to augment considerably the salary upon which a civil engineer in the service of the Indian Government will hereafter commence his work. The memorialists believe the proposed college will be prejudicial to the public service by narrowing the field for the selection of candidates, and by limiting their means of obtaining the requisite instruction. They submit that the working of Government colleges has not been such as to recommend the creation of a new Government college, having practically the monopoly of appointments, and protected from competition. They suggest that such a step is at variance with the plan for throwing open to all Her Majesty's subjects the opportunity of gaining Government appointments.

AT a meeting of the Royal Geographical Society, held on the 9th inst., Dr. Carpenter read a paper on Oceanic Circulation, especially in reference to the inset current into the Mediterranean through the Straits of Gibraltar. After detailing the theories which have hitherto been propounded to account for this current, all of which he pronounced to be unsatisfactory, Dr. Carpenter propounded the following explanation:—The water of the Mediterranean has a uniform but limited excess of weight over that of the Atlantic—so limited as to do away altogether with the idea that there is an accumulation of salt in the Mediterranean. The excess of salt was found to be greater in the lower than in the upper stratum of water, and thus the excess of evaporation was produced. If there be two columns of water of equal density—one that of the Atlantic, the other that of the Mediterranean—an excess of evaporation lowers the height of the Mediterranean column. If the Atlantic column were of fresh water, just enough would flow in from it to restore the evaporated water. But it is salt water which actually flows in from the Atlantic, and therefore it produces an increase of pressure, which presses the Mediterranean water outward till the equilibrium is restored. This being restored, there is once more a reduction of the Mediterranean level by evaporation, and so on; and, therefore, a circulation is always going on between the waters of the Mediterranean and Atlantic. The only difficulty in receiving this explanation is that the water of the outward-flowing current must run uphill; but other examples, he stated, are known. The fact that such a circulation exists, he said, is now indisputable, recent experiments having satisfactorily determined the evidence of a westerly current underlying the surface easterly current. These currents are, therefore, only a portion of the general oceanic circulation, which causes a perpetual surface motion of warm water from the equator to the pole, and a counter under-current of cold water from the pole to the equator. In replying to objections raised in the course of the discussion, Dr. Carpenter said that he had only broken ground in the enunciation of his theory, which it would require much labour yet to work out. Sir Henry Rawlinson, who occupied the chair, stated that in consequence of the continued illness of Sir R. Murchison, he had been appointed president of the Society *ad interim*.

THE Wind-Chart, to which we referred last week, furnished daily by the Meteorological Office to the *Shipping and Mercantile Gazette*, is the invention of Captain Charles Chapman.

FROM the annual report, it appears that the gardens of the Dublin Zoological Society have been very successful, the number of visitors being 136,000, and a considerable balance lies in favour of the society. The donations of axolotls from Prof. Wyville Thomson, and of Mammoth Cave fishes from Prof. Mapother, are acknowledged, but it is not stated if they live or have undergone any developmental changes.

THE following regulations have been issued by the Science and Art Department with regard to science teachers who wish to attend the special (six weeks') courses for training in teaching at South Kensington in June, July, and August, in accordance with the circular of September 1870. These applications are so numerous that it is impossible for the Department to make the selection of teachers to attend, as was originally proposed. The Department has therefore decided to make the selection by competition at the next May examinations. The competition for those who wish to attend the instruction in Biology will be in Animal Physiology, Zoology, Vegetable Anatomy and Physiology, and Systematic and Economic Botany. The candidate may take up all the four subjects, but a fair proficiency in Animal Physiology and in Vegetable Anatomy and Physiology will be essential. If this proficiency is shown, the marks obtained in the other subjects will be counted. By fair proficiency is understood the amount of knowledge required to pass in the advanced stage. For those who wish to attend in Experimental Physics, the competition will be in Acoustics, Light and Heat, and in Magnetism and Electricity. A candidate will be required to take up the honours' papers in those subjects in which he teaches, or in which he is qualified to earn payment, but in any of the other subjects of the group in which he is competing he may take the advanced or the elementary paper. The marks obtained in these subjects will count as in the competition for Royal Exhibitions. The papers of the teachers who are competing will be specially and separately looked over by the professional examiners—the examination being competitive only—and the teachers who answer best, probably to the number of

about forty-five in each group, will be allowed to come to London for the six weeks' course of training. They will receive their travelling expenses, namely, second-class railway fare, and 30s. a week while in London. The results of the teachers' examination will not be published. Each candidate will be informed of the position in which he would have been placed had he been examined as an ordinary candidate for honours. If he wish his success recorded, it will be done in the ordinary way. The Biology course will commence on the 14th June, and the course on Experimental Physics on the 5th July. It must be understood that no teachers, except such as come up under the foregoing rules, will have travelling allowances under § lxviii. of the Science Directory. No persons are eligible to receive the allowances granted in this minute except those who have been engaged in science teaching under the Science and Art Department during the session 1870-71.

IT appears from Mr. Glaisher's Meteorological Tables, that while December 25 was the severest day of the recent frost in the neighbourhood of London, it was still more intense in other localities on January 1, falling as low as 4·9° at Wolverhampton, 5·0° at Hull, 6·7° at Bradford, 7·0° at Leeds, and 9·7° at Leicester, the minimum at Blackheath on that day being as high as 19·2°, although at Twickenham, as reported by Mr. Hall last week, the thermometer fell as low as 6·3°. As on Christmas Day the severest cold was again in the Midland and North Midland counties, and we have once more to record the phenomenon to which we have had occasion to allude more than once recently, that the frost was considerably more intense in England than in Scotland. For the week ending January 7, no lower temperature was recorded at any of the Scotch stations than 21·0°, at Dundee; the mean temperature for all the English stations was 29·6°, while for all the Scotch stations it was 35·5°, or more than 6° higher. From the 20th of December to the 13th of January, when the frost finally broke up, the temperature in London was only even slightly above the mean of the last fifty years on three days.

THE Rev. Mr. Gribble, F.R.S., reports from Constantinople that after a fine morning the disc of the sun in the late eclipse was obscured for some time, so that it was not well seen until $3^h\ 26^m$ mean time, when there was only about one-eighth of the disc clear. A large solar spot then became uncovered; clouds coming on again, this was all that was seen clearly. At $3^h\ 26^m$ the light was considerable.

THE *Scottish Naturalist*, to which we referred some weeks since as a new periodical proposed to be issued under the auspices of the Perthshire Society of Natural Science, has made a good beginning. Opening with a short programme of future operations, in which the editor, Dr. Buchanan White, wisely states his intention of restricting the contributions to papers and notes especially bearing on Scottish Natural History; we then have an article by Dr. Lauder Lindsay "On Natural Science Chairs in our Universities," to which we may possibly take occasion again to refer. The writer deals with a bold hand with the evils which are said to prevail more especially in the election of Professors in our northern Universities. He refers to the manner in which these appointments are stated often to be regulated by political influence or religious belief rather than by possession of qualifications to teach Natural Science, and is especially severe on the system of personal canvass and the "testimonial nuisance." Mr. J. A. Harker contributes a paper "On the Work and Influence of Local Natural History Societies;" Mr. George Norman a List of the Noctuæ occurring in Morayshire; the editor some hints on Sugaring, how, when, and where to do it; and an anonymous contributor, a paper "On the Mollusca of the North-east of Scotland," illustrated with a map. Some shorter articles and notes with reports of proceedings of Scottish Natural History Societies, make up the number, which we commend to the notice of all interested in natural history across the Border.

HENDERSON'S PATENT PROCESS FOR REFINING CAST-IRON

AN article was furnished a short time ago, and published in NATURE (No. 57, p. 94), respecting a new process for the production of steel by the partial decarburisation of cast-iron, invented by the writer. It was then stated that a description of a new process for refining cast-iron would soon follow.

This process supplies a more effective and economical process than the English finery or the German reverberatory furnace processes for refining cast-iron. It is very simple, and does not require any fuel, labour, or expensive apparatus; and there is no loss of weight of iron, as the impurities go off in vapour. The cost of refining is less than one-twentieth of that of the English and German processes; and its effect is more thorough than is possible by those systems.

The agents used are fluorine and oxygen combined. The fluorine is derived from any fluoride, and the oxygen from any substance containing or capable of evolving oxygen, which is adapted to use in the manufacture of wrought-iron and steel. Fluorspar and pure rich iron ores are the most available and economical substances for producing these agents, and are applied finely powdered and mixed, and placed in receptacles, preferably, so as to act from the under side upwards upon cast-iron in its molten state.

The most economical mode of application of this process is to treat the cast-iron in the condition in which it flows from the blast furnaces, with fluorspar and iron ore, applied in the "chills" or pig moulds used at blast furnaces, by being spread over the bottom of the moulds.

The iron, when tapped from the blast furnace, flows into the mould thus prepared; the heat of the iron causes fluorine and oxygen to be liberated, and, by reason of the affinities of these substances for silicon and phosphorus, these impurities are removed in the form of *vapour*. The reactions in the "chills" are similar to those of the boiling puddling process, and last about five minutes. The metal during this period is covered with jets of flame and smoke. The resulting metal, with respect to silicon and phosphorus, is as pure as wrought-iron.

It is preferable to use iron ores containing the largest amount of oxygen and the least of silica and phosphorus. These conditions exist in "washed iron sands," and the red hematites of Cumberland and Lancashire. When using the hematite ores, varieties that are the easiest to reduce to powder are preferred; and the ordinary edge running apparatus, with cast-iron rollers revolving in a pan, is an excellent one for the purpose.

The fluorspar and fine ore are passed through a sieve of not less than four hundred meshes to the square inch, and afterwards mixed so thoroughly as to appear to be one substance, in the proportion of one part of fluorspar to two parts by weight of iron ore, and are spread one-fourth to three-eighths of an inch deep over the "chills," then the iron is run upon them so as to form slabs one inch thick.

The former article gave analyses of refined cast-iron produced in the reverberatory furnace. It will be seen by comparing the analysis of the refined metal of this process with the analysis of that of the reverberatory furnace process, that they are analogous, except that the latter contains no silicates or graphite. The refined metal of the pig-mould process saves the fuel and time taken for refining by the reverberatory furnace process, and shortens the time of producing steel or wrought-iron by that process fully forty minutes. When using the refined metal of this process two workmen can take charge of five ordinary boiling puddling furnaces making steel, and six furnaces making wrought-iron, as the only labour necessary is "balling" and removing it from the furnace. When high carburised steel is made, "balling" is not required, as

the metal will be fluid enough to run from the furnace into ingot-moulds. The economy of the process consists in the saving of the labour of eight workmen making steel, and two workmen making wrought-iron; and in the superior quality of the result as compared with puddling the refined metal. The time occupied by the conversion of the refined metal into steel or wrought-iron by the process without puddling is about the same as that usually taken in puddling pig-iron.

Ordinary coke pig-iron, smelted near Pittsburgh from a mixture of hematite ore and mill cinder, has been treated by this process. The refined metal was afterwards puddled and rolled into "muck" bars, and once heated and rolled into merchant bar-iron. The pig, refined cast, and wrought-iron, have been analysed by an analytical chemist in this city. These analyses are annexed, also analyses of refined cast-iron by the English finery and German reverberatory furnace process, and of the highest standard qualities of English, French, Swedish, and Russian wrought-iron, are given for comparison, taken from Percy's "Iron Metallurgy" and Ure's Dictionary:

	Pittsburgh Coke Pig-iron.	Patent Refined Cast-iron.	Bar-iron.
Carbon (combined)	0·2040	0·3613	not deter.
do (graphite)	2·7685	2·5066	—
Silicon	2·3096	none	none
Slags (silicates)	0·3623	0·2983	not deter.
Phosphorus	0·4196	0·1029	0·0087
Sulphur	0·1298	0·1269	0·0438

Analyses of Foreign Iron.

	English Finery Process. Refined Cast-iron.	German Reverberatory Furnace Process. Pig-iron.	German Reverberatory Furnace Process. Refined Cast-iron.
Carbon	3·07	—	—
Silicon	0·63	4·66	0·62
Phosphorus	0·73	0·56	0·50
Sulphur	0·16	0·04	0·03
Silica, Alumina (Silicates)	0·44		

	English Wrought-iron. Low Moor Stamp.	French. Petin, Gaudet, & Co.	Swedish. Hoop L.	Russian. CCND
Carbon	0·016	0·143	0·087	0·272
Silicon	0·122	—	0·115	0·062
Sulphur	0·104	0·058	0·220	0·234
Phosphorus	0·106	0·030	0·034	—

On comparing the analysis of the refined cast-iron of the patent process with those of the English and German processes, it will be seen that while the refined iron of the new process contains *no silicon*, that of the English and German processes contains 0·63 and 0·62 per cent. respectively; and compared as regards phosphorus, the German process reduces it from 0·56 to 0·50 per cent., or about 0·06 per cent., and the new process reduces it 0·32 per cent., or over five times as much phosphorus is removed by the new process as by the other processes. The slags or silicates are 0·15 per cent. less than in the refined iron of the English finery process. The analysis of the patent refined cast-iron, compared with that of the above English wrought-iron, shows that whilst the wrought-iron contains 0·122 per cent. of silicon, the refined metal contains none, and compared as regards phosphorus they are about the same.

The analysis of the puddled wrought-iron, made from the refined cast-iron by once heating and rolling the puddled or "muck" bar, shows a purer quality of iron than the most celebrated makers of Europe produced from the purest ores with charcoal.

The economy of using the refined metal in saving cost of labour, fuel, &c., for the puddling process, has been fully demonstrated by numerous trials. When all the advantages of the process are realised, about one-half of the cost of converting cast-iron into wrought-iron can be saved; and there is an improvement in the quality equal to the difference between ordinary forge pig-iron and charcoal iron.

These advantages are:—

1. Better quality, which is due to the purity of the refined metal; as good qualities of wrought-iron are produced from it as from pig-iron made from the best ores smelted with charcoal.

2. The refined metal being as pure as wrought-iron, with respect to silicon and phosphorus, requires merely decarbonising, with less skill to work it, and greater certainty of the quality of the product.

3. Large saving in cost of production, owing to the shortening of the time of puddling, which is caused by the removal of a large part of the impurities by the refining process. White refined iron is decarbonised in twelve to fifteen minutes, and a "heat" or charge of five hundred pounds is puddled in fifty-five minutes, including time of charging, melting the iron, and stirring or puddling or "balling" and removing it from the furnace; grey forge iron requires sixty-five minutes, and foundry iron about seventy minutes. Seven "heats" or charges to a "turn" or a day's labour, are of easier accomplishment than five charges are from the pig-iron from which it was produced; the five charges now require ten hours to convert pig-iron into wrought-iron. It is possible to obtain, with the patent refined metal, by employing three sets of workmen in twenty-four hours, instead of two sets as is now customary, twenty-one charges in twenty-four hours instead of ten charges; and allowing sufficient time for repairs, the production of any ironwork may be doubled, without additional investment of capital and without additional cost of repairs.

4. Saving of fuel per ton of iron produced, amounting to one-half, caused by increased production.

5. Reduction of general business expenses per ton of iron, amounting to one-half, caused by increased production.

6. Reduction of wages, by reason of the diminished labour, of 40 per cent. per ton of iron.

7. The puddling furnace cinders of the refined metal contain but about one-fourth of the phosphorus, as compared with the cinders resulting from the use of pig-iron; and when smelted produce better qualities of pig-iron.

The cost of refining in the pig-moulds is very little. Fluorspar is a cheap material, and but about seventy pounds are required to refine a ton of iron. The cost is nearly compensated by the saving of the fuel and lime which would be required to reduce the puddle cinders of the refined cast-iron to pig-iron, as they contain but small portions of silica, and will require less fuel and limestone. The residue of the fluorspar and oxide is agglutinated, and remains in the pig moulds, and is pure lime and deoxidised iron ore, and is available as so much lime and ore in the blast furnace. JAMES HENDERSON

SOME EXPERIMENTS ON COLOUR

THE theory of colour perception, although in England it has not yet made its way into the text-books, still less into the popular works on science, is fully established with regard to many important points. It is known that our perception of colour is threefold, that is, that any colour may be regarded as made up of definite quantities of three primary colours, the exact nature of which is, however, still uncertain. More strictly stated, the fundamental fact in the doctrine of colour is that, between any four colours whatever given, as well in quantity as in quality, there exists what mathematicians call a linear relation, that is, that either a mixture of two of them (in proper proportions) can be found identical, so far as the eye is able to judge, with a mixture of the other two, or else that one of them can be matched by a mixture of the other three. There are various optical contrivances by which the mixture spoken of may be effected. In the year 1857, Mr. Maxwell published an account of some experiments with the colour top undertaken to test the theory. From six coloured papers, black, white, red, green, yellow, and blue, discs of two sizes were prepared, which were then slit along a radius so as to admit of being slipped one over the other. Any five out of the six being taken, a match or colour

equation between them is possible. For instance, if yellow be excluded, the other five must be arranged so that a mixture of red, green, and blue is matched with a mixture of black and white. The large discs of the three colours are taken and slipped on to each other, and similarly the small discs of black and white. When the small discs are placed over the others and the whole made to rotate rapidly on any kind of spinning machine, the colours are blended, those of the large discs and those of the small, each into a uniform tint.

By adjustment of the discs an arrangement may be found after repeated trials, such that the colour of the inner circle is exactly the same both in tint and luminosity with that of the outer rim. The quantities of each colour exposed may then be read off on a graduated circle, and the result recorded. For instance (the circle being divided into 192 parts), eighty-two parts red mixed with fifty-six green and fifty-four blue, match thirty-seven parts white mixed with 155 black. In this way Maxwell observed the colour equations between each set of five, in all six sets formed by leaving out in turn each of the six colours. Moreover, for greater accuracy each set was observed six times, and the mean taken. But according to the theory these six final equations are not all independent of each other, but if any two of them are supposed known, the others can be found by a simple calculation. Accordingly, the comparison of the calculated and observed equations furnishes a test of the theory: but in practice, in order to ensure greater accuracy, instead of founding the calculations on two of the actually observed equations chosen arbitrarily, it is preferable to combine all the observations into two equations, which may then be made the basis of calculation. In this way, a system of equations is found necessarily consistent with itself, and agreeing as nearly as possible with the actually observed equations. A comparison of the two sets gives evidence as to the truth of the theory according to which the calculations are made, or if this be considered beyond doubt, tests the accuracy of the observations. In Maxwell's experiments the average difference between the calculated and observed systems amounted to ·77 divisions of which the circle carried 100. So good an agreement is regarded by him as a confirmation of the whole theory; but it seems to me, I confess, that only a very limited part of it is concerned. The axioms, in virtue of which it is permitted to combine the colour equations in the manner required for the calculations, are only such as the following:—If colours which match are mixed with colours which match, the results will match. It is difficult to imagine any theory of colour which will not include them. What proves the threefold character of colour—the most important part of the doctrine—is simply the fact that with any five-coloured papers *whatever* a match can be made, while with less than five it cannot (except in certain particular cases). In regard to this point the value of the quantitative experiments is rather that they show of what sort of accuracy the eye is capable in this kind of observation. Those to whom the subject is new may think at first that if colour be threefold a match ought to be possible between any *four* colours. And so it is possible if there is no other limitation; but in experiments with the revolving discs, we are subject to a limitation, being obliged to fill up the whole circumference somehow. The difficulty will clear itself up, when it is remembered that one of the five colours may be black, so that with any *four* colours and *black* a match can be made with revolving discs.

It was rather for my own satisfaction than with the hope of adding anything new to a subject already so fully and ably treated by Maxwell, that I commenced a repetition of his experiments. The colours used were, roughly speaking, the same as his, as was also the general plan of the observations. The agreement of the calculated and directly observed equations was very good, the average error being only ·24 divisions of which the complete circle contained ninety-six, or one-third of the corresponding average error in Mr. Maxwell's Table. A second set of observations and calculations made after a year's interval with a different set of colours gave about the same result. I am inclined to attribute the considerably greater accuracy of my observations rather to an excellent perception of minute differences of colour (to which I have always found my eyes very sensitive) than to greater care in conducting the experiments. One precaution, however, I have found so important as to be worth mentioning. Unless the small discs are very accurately cut and centred, a coloured rim appears on rotation between the two uniform tints to be compared and adjusted to identity, which is exceedingly distracting to the eye, and interferes much with the accuracy of the comparison. One set of observations made with the same care, and apparently as satisfactory as any of the others, puzzled me for some time on account of the great discrepancies with the others which it exhibited. I have no doubt that the cause lay in the different character of the light on the day in question, which came from the unusually blue sky which sometimes accompanies a high wind. On the other days the light came principally from clouds. I have had no opportunity of confirming this opinion by a repetition of the experiment with a sky of the same degree of blueness, but that the disagreement was not the result of unusually large errors of observation, is, I think, to be inferred from the fact that the observations under the blue sky were as consistent among themselves as any of the other sets. As the point is of some interest, I give the figures in full.

July 23, blue sky.

Black.	White.	Red.	Green.	Yellow.	Blue.	
0	+ 30	+ 122	+ 40	— 77	— 115	obs.
0	+ 32·2	+ 120·8	+ 39·1	— 78·8	— 113·2	calcd.
+ 94	0	— 132	— 60	+ 55	+ 43	obs.
+ 91·6	0	— 133·5	— 58·5	+ 54·4	+ 45·9	
— 138	— 54	0	+ 24	+ 50	+ 118	obs.
— 138·3	— 53·7	0	+ 23·1	+ 49·5	+ 119·5	calcd.
+ 92	+ 50	+ 50	0	— 66	— 126	obs.
+ 94·1	+ 49·5	+ 48·5	0	— 65·2	— 126·7	calcd.
— 154	— 38	+ 86	+ 52	0	+ 54	obs.
— 154·6	— 37·5	+ 84·6	+ 53·1	0	+ 54·3	calcd.
+ 139	+ 18	— 128	— 64	+ 35	0	obs.
+ 138·5	+ 19·7	— 127·5	— 64·5	+ 33·9	0	calcd.

The numbers read off for the big discs are written with the sign + prefixed, and those corresponding to the little discs with —. Thus the first line may be read: — 30 parts white together with 122 red and 40 green, match 77 yellow and 115 blue. The upper line of each pair represents the actual observation, and the second is the theoretical equation calculated from two in the manner described. The average difference between the two sets of numbers which may be taken as a measure of the inaccuracy of the observations amounts to 1·1. A similar table, formed from the observations of July 20 (cloudy), and which agreed very well with the results of other days, is as follows* :—

Black.	White.	Red.	Green.	Yellow.	Blue.
0	+ 30	+ 117	+ 45	— 79	— 113
0	31·1	116·2	44·8	79·9	112·2
+ 90	0	— 128	— 64	+ 56	+ 46
85·9	0	128·4	63·5	57·0	49·0
— 136	— 56	0	+ 22	+ 52	+ 118
137	55	0	22·3	50	119·6
+ 100	+ 50	+ 42	0	— 64	— 128
99·2	51	41·9	0	65	127·1
+ 135	+ 21	— 123	— 69	+ 36	0
135·7	21·5	122·7	69·3	34·8	0
— 152	— 40	+ 80	+ 56	0	+ 56
152·6	39·5	81	56	0	55

The average error is here ·95, showing only a trifling better agreement than the former set, so that the blue sky observations are nearly as self-consistent as those made with cloud-light. Moreover, the agreement is itself very good, being decidedly better than Maxwell's, though his calculations refer to a *mean* of six sets of observations.

While therefore there is no reason to distrust the results of July 23 any more than of July 20, the differences between them are much greater than can be ascribed to errors of observation. It will be found that they relate principally to the quantities of red, the numbers under that head being considerably greater for the case of the blue light from the sky. I am not aware whether the difference of sky and cloud light has ever been made the subject of direct investigation, but it would seem a fair inference that it must consist mainly in a relative deficiency of the red rays. If this be so, as I have other grounds for suspecting, the light of the

*** These calculations were made by means of Prof. Everett's Proportion table, which seems admirably adapted to work of this sort.**

sky would be similar in composition to that of dilute solutions of copper, which acquire their light blue tint by a partial suppression of the extreme red.* There is no doubt that the colour equations are dependent on the character of the light, as may easily be proved by taking an observation looking all the time through a layer of coloured liquid. It is not, however, the most brilliantly coloured solutions that cause the most disturbance, for anything like a complete stoppage of all the rays which are capable of exciting one of the primary colour sensations would affect both the mixtures to be compared in nearly the same manner, putting the observer in fact very much into the positions of a colour-blind person. Those liquids will be most efficient which have a different action on parts of the spectrum allied in colour. For instance, an aqueous infusion of litmus has a strongly marked action on the yellow ray, stopping it with great energy, even in rather dilute solutions. It is easy to trace the effect of looking through this on most of the colour equations. Consider, for example, the fifth equation of July 20 (that from which the blue is absent) wherein red and green are matched against black, white, and yellow. The red and green will for the most part escape absorption, but the white and yellow will be shorn of a part of their yellow rays. The match supposed to have been adjusted without the litmus must evidently be spoiled; the red and green mixture becoming strongly yellow in comparison with the other. In order to restore equivalence the yellow must be considerably increased. On trial I found, 124 black + 19 white + 49 yellow matched 121 red + 71 green.

It is only the impurity of the colours on the discs that prevents the effect being still more strongly marked, for with the pure colours of the spectrum the most violent alterations are possible. When a match is made between the simple yellow and that compounded of pure red and green, almost any coloured liquid acts unequally on the two parts and destroys the balance. The simple yellow, of course, retains its colour under any absorbing influence, and can only be changed in luminosity. Chloride of copper extinguishes the red component of the compound yellow, which accordingly becomes green. Litmus would leave the compound colour nearly unchanged, while it extinguishes the simple yellow. It is needless to multiply instances.

Before leaving the compound yellow, of whose very existence many are incredulous, I will mention an easy way of obtaining it, which is the more desirable as the use of the pure spectral colours is not very convenient. In order to isolate the red and green rays of the spectrum by means of absorption, the first thing is to find a liquid capable of removing the intermediate yellow and orange. With this object we may fall back on the alkaline solution of litmus, whose opacity to the yellow, and particularly to the orange rays is so marked. The next step is to remove the blue and bluish green, for which nothing is more convenient than the chromate of potash. A mixture of these two liquids in proper proportions, easily found by trial, isolates the green and extreme red rays with considerable perfection, and exhibits in a high degree the phenomenon of Dichromatism. According to the thickness traversed by the light the red or the green predominates, and there is no difficulty with a given thickness in arranging the strength of the solution so as to give a full compound yellow. It is worth notice in confirmation of the opinion expressed as to the character of the sky-blue, that when a cloud seen through the liquid appears a full yellow, or even orange, the former, if at all intense, acquires a decided green colour. A window backed by well-lighted clouds, when looked at across a room through the liquid and a prism, has a very splendid appearance, the red being isolated on one side, and the green on the other; while the intermediate space, where the two overlap, shows the compound yellow in great perfection. Another liquid, in some respects preferable, which answers the same purpose, may be made by mixing chloride of chromium and bichromate of potash. Through either of them the sodium flame is invisible, though they may easily be made to correspond with it in colour very closely. I tried to obtain a liquid capable of isolating the pure yellow ray, but only with partial success. The best was a mixture of bichromate and permanganate of potash with a salt of copper (sulphate or chloride). The first removes the blue and violet, the second the green, and the third the red, and thus the yellow is isolated in considerable purity. This liquid is very unstable. The comparison of the simple and compound yellow (which nearly matched) was interesting. One was transparent to the sodium flame, the other completely opaque to it. When the two are brought together so that the light has to traverse both, almost complete darkness results, even when the brightest clouds are used. I should mention that it is only when the light is strong that any of these liquids give yellow in full perfection; otherwise the colour is more nearly described as brown, which is, in fact, identical with a dark yellow or orange. The best natural yellows, such as chrome, are partly simple and partly compound, returning all the light which falls upon them except the blue and violet. It is clear that neither a purely simple nor a purely compound yellow can rival them in brilliancy.

Impartial observers, unprejudiced by the results of mixing pigments, or, on the other hand, by experiments on the spectrum, see, so far as I can make out, no connection between the four principal colours—red, yellow, green, and blue. It seems to them quite as absurd that yellow should be compounded of red and green, as it most unquestionably is, as that green should be a compound of blue and yellow, though many have accepted the latter alternative on the authority of painters, and some have even worked themselves into the belief that it is only necessary to look at the colours in order to recognise the compound nature of green. My own prejudice would be on the other side, the result of experiments on the compound yellow, which is seen so easily to pass into green on the one side or red on the other. The most impartial opinion that I can form is that there is no real *resemblance* between any of the four, and if this be so it is certainly a most remarkable, if not unaccountable, fact. The difficulty is not so much that we are unable to analyse the compound sensation, as to explain why our inability is limited to yellow (and white). For everyone, I imagine, sees in purple a resemblance to its components red and blue, and can trace the primary colours in a mixture of green and blue. Sir John Herschel even thinks that our inability to resolve yellow leaves it doubtful whether our vision is trichromic or tetrachromic, but this seems to me to be going much too far. Surely the fact that the most saturated yellow can be compounded of red and green, deprives it of any right to stand in the same rank with them as primary colours, however little resemblance it may bear to them and blue. Besides, if yellow is to be considered primary, why not also white, which is quite as distinct a sensation as any of the others? Undoubtedly there is much that is still obscure in the mutual relations of the colours—why, for instance, as mentioned by Sir John Herschel, a dark yellow or orange suggests its character so little as to be called by a new name (brown), while a dark blue is blue still. But difficulties such as these should make us all the more determined to build our theories of colour on the solid ground that normal vision is threefold, and that the three primary elements of colour correspond nearly with red, green, and blue.*

J. W. STRUTT

* Direct observations, made since the above was written, show that there is no *peculiar* deficiency at the red end of the spectrum, but a general falling off as the refrangibility diminishes from one end to the other. If lights from sky and cloud are of equal intensity at the line C in the red the first will be somewhere about twice as bright as the other at B in the green. This is for a well-developed blue light taken from the zenith; but, even with a large allowance, enough difference remains to account for the discrepancies in the two sets of colour disc observations. I have lately found from theory that the power of very small particles to scatter the rays belonging to different parts of the spectrum varies as the inverse fourth power of the wave length.

* This paper was read before Section A of the Liverpool Meeting of the British Association.

SCIENTIFIC SERIALS

THE *Quarterly Journal of Science* for January commences a new *régime* under the sole editorship of Mr. W. Crookes. As will be seen from the following summary of its contents, all the papers, with only one exception, refer to some department of Physical Science to the exclusion of Natural History. The articles are as follows:—1. "Double Spectra," by W. M. Watts. A *résumé* of the facts known to the present time to modify the conclusions drawn from the earlier spectrum researches of Bunsen and Kirchhoff, from which it was concluded that the spectrum of each element was one and invariable. A plain and a coloured lithographic illustration show the spectrum of copper chloride when volatilised undecomposed, as contrasted with that of the metal; the different spectra of barium, strontium, and calcium obtained at different temperatures; the three spectra of hydrogen, $H\alpha$, $H\beta$, and $H\gamma$, probably due to differences in temperature; the two spectra of aluminium; the two of nitrogen; and the four spectra which are all probably due to incandescent carbon vapour. 2. "The Great Pyramid of Egypt, from a modern scientific point of view," by C. Piazzi Smyth, Part I. 3. "On the Theory of

Irrigation," by F. C. Danvers. An account of the advantages that would result to agriculture from a system of irrigation, rendered necessary by the destruction of forests, irrespective of the question of the utilisation of sewage. 4. "War Science," by H. Baden Pritchard. A statement of the instruction in War Science given in the Royal Artillery and Royal Engineers, and of recent improvements in gunnery, illustrated especially by the Scott and Moncreiff gun-carriages; the improved modes of manufacturing gunpowder; and the invention of the electric torpedo in conjunction with the Abel fuse. 5. "Spectra of Metallic Compounds," from the Journal of the Franklin Institute. 6. "On the various Tints of Autumnal Foliage," by H. C. Sorby. A very interesting paper, in which the writer details the results of his experiments on the various colouring matters of leaves, &c., which he classifies as follows:—(1) *chlorophyll*, or the green colouring matter; very rarely found pure, even in fresh leaves; insoluble in water, but soluble in alcohol or bisulphide of carbon; the spectra have all a very well-marked absorption band in the red, but the green is more or less completely transmitted, so that the prevailing tint is a more or less modified green; (2) the *xanthophyll*, or yellow group; insoluble in water, but soluble in alcohol and in bisulphide of carbon; the spectra show absorption at the blue end, often with more or less well-marked narrow bands; but the red, yellow, and yellow-green are freely transmitted, so that the general colour is clear yellow or orange; (3) *erythrophyll*, or the red colouring matter; soluble in water and aqueous alcohol, but not in bisulphide of carbon; show strong absorption in the green part of the spectrum; (4) *chrysophyll*, or the golden-yellow group; soluble in water and aqueous alcohol, but insoluble in bisulphide of carbon, with variable spectra; (5) the *phaiophyll* group, comprising various browns; soluble in water, but not in bisulphide of carbon; do not give well-defined absorption-bands. Mr. Sorby gives the following scheme of the relative abundance of these various groups of colours as the ieaves advance towards decay.

Complete vitality and growth . .	Chrysophyll . .	More or less bright green.
	Chlorophyll . .	More or less green-brown.
Low vitality and change . . .	Erythrophyll . .	More or less red-scarlet.
	Xanthophyll . .	More or less bright orange-brown.
Death and decomposition . .	Phaiophyll . .	
	Humus	Less or more dull brown.

7. "On the Relations between Chemical Change, Heat, and Force, with a special view to the economy of electro-dynamic engines," by the Rev. H. Highton. Contests the theory generally accepted that a certain amount of chemical change corresponds and is interchangeable with a certain amount of heat and electric force; and that this heat again corresponds and is interchangeable with a certain amount of work or mechanical energy. The author considers that the whole subject requires a fresh, strict, and full experimental investigation. He then refers to the many different answers that have been given to the question, What is the mechanical equivalent of heat? and holds that it has never been proved that there is any such equivalent. He details in support of his view the conflicting results obtained from the elaborate experiments with a galvanic battery by such experimenters as M. Favre, M. Sorel, M. Weber, M. Kohlrausch, and Mr. Gore. 8. "Our Patent Laws."—The pages devoted to "Progress of the Sciences" again embrace Physical Science only, Physics, Chemistry, and Mechanics—an inequality which will doubtless be rectified in future numbers.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 12.—"Some Experiments on the Discharge of Electricity through Rarefied Media and the Atmosphere." By C. F. Varley.—After the labours of Mr. Gassiot, one approaches this subject with diffidence, lest he should appear to be attempting to appropriate the glory which so justly belongs to that gentleman and to Professor Grove. The nature of the action inside the tube is at present involved in considerably mystery, but some light is thrown upon the subject by the following experiments. Before describing them, however, the author wishes to observe that he has seen Mr. Gassiot's last papers,* and finds that, so far as regulating the strength of the current is concerned, he has been proceeding in a similar manner to the author. The tube principally used in these experiments contains two aluminium wire rings, the one $\frac{4}{10}$ inch in diameter, the other $\frac{8}{10}$, and separated $\frac{8}{10}$ inch, the tube 1¼ inch in diameter, 3¼ inches in length; it was one of Geissler's manufacture, was very well exhausted, and professed to contain hydrogen. A U-shaped glass tube containing glycerine and water was placed in circuit. Two aluminium wires inserted in this tube gave a ready means of reducing or augmenting the resistance at pleasure. Glycerine affords an easy means of producing very great resistances. The battery used in this experiment was a Daniell's battery, each cell having a resistance of from 50 to 100 Ohms. The resistance of the glycerine-and-water-tube was between 2 and 3 megohms; this latter resistance was made large, in order that the resistance of the tube and battery might be neglected without entailing error. The following law was found to govern the passage of the current:—1st. Each tube requires a certain potential to leap across. 2nd. That having once established a passage for the current, a lower potential is sufficient to continue the current. 3rd. If the minimum potential, which will maintain a current through the tube, be P, and the power be varied to $P+1$, $P+2$, &c. to $P+n$, the current will vary in strength, as 1, 2, &c. n. The following Tables (I. and II.) illustrate this; there is a little irregularity in the figures due to the irregularity of the battery, although it was recharged for the occasion.

TABLE I.

1.	2.				3.	4.
Cells of Daniell's Battery, $P+n$.	Observed Deflections of Reflecting Galvanometer.				Mean.	3rd Col. divided by n.
307 = P + 3	0	0	0	0	0	0
308 = P + 4	5	5½	5	5½	5¼	1·3
309 = P + 5	9	9	9	9	9	1·8
310 = P + 6	12	12½	12	12½	12¼	2·04
311 = P + 7	14	14	..	..	14	2
312 = P + 8	16	..	..	..	16	2
313 = P + 9	17½	18	18	..	17¾	1·97
314 = P + 10	19½	19½	..	..	19½	1·95
315 = P + 11	21½	21	22	21½	21½	1·95
316 = P + 12	23½	23½	23½	..	23½	1·96
317 = P + 13	25½	..	..	..	25½	1·96
318 = P + 14	27½	..	..	..	27½	1·97
319 = P + 15	29½	..	..	..	29½	1·97
320 = P + 16	31½	31½	31	31	31¼	1·95
*323 = P + 19	37½	38	37	37½	37½	1·97
325 = P + 21	40¾	41	40½	..	41¾	1·94
330 = P + 26	51	51	..	..	51	1·96
335 = P + 31	60½	60½	..	..	60½	1·95
340 = P + 36	70	70	..	..	70	1·94
345 = P + 41	79½	79½	..	..	79½	1·94
350 = P + 46	89	89	..	..	89	1·94
355 = P + 51	98½	98½	..	..	98½	1·93
360 = P + 56	108	108	..	..	108	1·93
365 = P + 61	118	118	..	..	118	1·94
370 = P + 66	128	128	..	..	128	1·94
375 = P + 71	139	140	..	..	1·9½	1·96
380 = P + 76	150	150	..	..	150	1·97

TABLE II.

1.	2.				3.	4.
304 = P + 0	0	..	..	..	0	0
305 = P + 1	2	2	..	..	2	2
306 = P + 2	4	4	..	..	4	2
307 = P + 3	6	6	..	..	6	2
308 = P + 4	8	8	..	..	8	2
309 = P + 5	10	10	..	..	10	2
310 = P + 6	12	12	..	..	12	2
320 = P + 16	31½	32	..	..	31¾	1·97
330 = P + 26	51	51	..	..	51	1·96
340 = P + 36	71	71½	..	..	71	1·97

It thus appears that a certain amount of power is necessary to spring across the vacuum; after that it behaves as an ordinary conductor, excluding that portion of the battery whose potential is P, and which is used to balance the opposition of the tube. In these experiments P was 304 cells. The tube in question could not be persuaded to allow a current of less than 323 cells to pass; but when once the current had established a channel, on lowering the potential by *short circuiting* portions of the battery, so as not to break the circuit, the current would flow when the battery was reduced to 308 cells. By, however, passing a current from 600 cells through the second tube W, filled with pure glycerine, and offering several thousand megohms resistance, an extremely feeble current, too weak to affect the galvanometer, kept a channel open by its passage; with this arrangement the figures in Table II. were obtained, which are more regular at the commencement, and a power of $P+1$ would pass

* Proc. Roy. Soc., vol. xi. p. 329, & vol. xii. p. 329.

* This power, 323, was the lowest at which the current would jump.

across the tube. The positive pole alone was observed to be luminous when the current was very minute, and the negative only was luminous when the current was strong. The following experiments were tried, and the results, which have been photographed, accompany the paper. A current was passed through the tube U and the vacuum; the U tube contained pure glycerine, and had a very large resistance, which was gradually reduced. At the commencement it was more than 10,000 megohms; the upper or small ring was positive, the lower ring was negative. The power was so reduced that the faintest possible light only was visible; in this case the positive wire alone was luminous, whether it were the large or small ring that was positive. The light was so feeble that, though the experiment was conducted in a perfectly dark room, we were sometimes unaware whether the current was passing or not. An exposure of thirty minutes' duration left, as will be seen, a very good photographic record of what was taking place; this means of viewing light too feeble for the eye may receive other applications. The resistance was then reduced, when the light became much more brilliant,—a tongue of light projected from the positive pole towards the negative, the latter being still almost completely obscure. The light around the positive pole was to all of our eyes white, while the projecting flame was a bright brick-red. This bright brick-red, however, possessed great photographic power. The negative wire at this stage began to show signs of luminosity. As the power was increased, the flame became detached from the positive pole. On still further increasing the power, the positive pole ceased to be luminous; and on still further increasing the power, by removing the U tube altogether, the light surrounded the negative wire, the photograph shows a white flattened hour-glass, apparently detached from the wire; to the eye, however, the wire appeared to be surrounded by a bright blue envelope $\frac{1}{4}$ inch in diameter, which did not possess sufficient photographic power to leave a record of itself, while the red portion did so. A large condenser was now attached to the battery, and discharged through the tube (the condenser had a capacity of 27 microfarads); this was equivalent to a momentary contact with a battery of little or no resistance. The flash was exceedingly brilliant to the eye; it could be heard outside the tube with a sharp click; the eye, however, was so dazzled as not to be able to see its shape. The light was confined entirely to the *positive pole;* thus, then, as the power is increased from nothing upwards, the first pole to become luminous is the positive; secondly, the two poles become luminous; thirdly, the negative pole alone is luminous; and fourthly, with an instantaneous discharge, the positive pole only is luminous. When the resistance in the U tube was greatly reduced, and a galvanometer (not very sensitive) was inserted, so that the chief resistance in circuit was that of the exhausted tube, as the potential was augmented cell by cell, the changes took place abruptly and suddenly. For example, when the power was so low that the positive pole only was visible, the current was feeble, and kept augmenting in power as cell after cell was added on. Suddenly the luminous red flame made its appearance, and the galvanometer showed that the current had suddenly augmented three or four times in power. As the power was again further increased, cell by cell, the current again steadily augmented in proportion, until the luminous tongue suddenly disappeared, the galvanometer showing a still further sudden increase in the current.

Nature of the luminous cloud.—Plücker has shown that when such an exhausted tube, with a current through it, is placed between the poles of an electro-magnet, a luminous arch is produced, which arch follows the course of the magnetic rays. As the electro-magnet is magnetized, the tube, which before was full of a luminous cloud, is seen gradually to change; the magnet gathers up this diffused cloud, and builds up an arch. Inasmuch as the electricity was passing in a continuous current from the battery, from wire to wire, it is evident the light is projected right and left into those parts of the tube where there is no electric current flowing. To endeavour to ascertain the nature of this arch, a tube was constructed. A piece of talc, bent into the form U, had a fibre of silk stretched across it; on this fibre of silk was cemented a thin strip of talc, 1 inch in length, $\frac{1}{16}$ inch broad, weighing about $\frac{1}{16}$ of a grain. The tube was sealed up and exhausted; carbonic acid and potash were used to get a high vacuum. When the magnet was not magnetized, the passage of the current from wire to wire did not affect the piece of talc. When the magnet was charged, and the luminous arch was made to play upon the lower portion of the talc, it repelled it, no matter which way the electric current was passing. When the tube was shifted over the poles of the magnet so as to project the luminous arch against the upper part of the talc, the upper end of the talc was repelled in all instances; the arch, when projected against the lower part of the talc, being near the magnet, was more concentrated, and the angle of deviation of the talc was as much as 20°. When the upper part of the arch, which was much more diffused, was thrown upon the upper part of the talc, it was repelled about 5°. This experiment, I think, indicates that this arch is composed of *attenuated particles of matter projected* from the negative pole by electricity in all directions, but that the magnet controls their course, and these particles seem to be thrown by momentum on each side of the negative pole, beyond the limit of the electric current. This arch requires time for its formation, for when a charged condenser is discharged through the tube no arch is produced. The arch from the negative pole is a hollow cylinder; the little talc tell-tale against which the arch was projected cut out the light, and a corresponding dark space existed throughout the remainder of the course of the arch. There was on the talc, at the spot where the arch struck it, a little bright luminous cloud, as though the attenuated luminous vapour was condensed by this material obstruction. Great care had been taken not to let the arch strike the single filament of silk which suspended the talc. Having demonstrated that the talc was repelled as described, the arch was allowed to play against the silk fibre, which the author expected would have been instantly burnt; such, however, was not the case. Even when a powerful induction-coil replaced the battery, the fibre remained unhurt.

Comparison of the above phenomena with discharges between the poles of a Holtz's Machine in air.—In the first part of this paper four different kinds of discharges were described *in vacuo.* With a "Holtz's" machine, which will give 11-inch sparks in the air, four well-marked different kinds of discharge have been obtained *in the air;* one of which, the author thinks, will explain the curious and rare phenomenon known as "ball lightning." In the experiments hereafter referred to, the condensers were in all cases attached to the "Holtz's" machine. The first discharge is the long 11-inch zigzag spark or lightning-flash; the second is the well-known "brush," which is best obtained by connecting the negative pole of the "Holtz's" machine to the earth; the third kind of discharge is a hissing red flame, $\frac{1}{2}$ inch in length, playing about the negative pole, the positive pole being scarcely luminous at all, and if luminous, at one or two points only; the fourth or most remarkable phenomenon is best obtained in the following manner (I should here remark that the brass balls on each of the poles are about an inch in diameter):—Tie to the negative pole a small thin strip or filament of wood three inches in length, and bent so as to project on each side of the negative pole, and a little beyond it towards the positive. On rotating the machine, two bright spots are seen upon the positive pole. If the positive pole be made to rotate upon its axis, the luminous spots do not rotate with it; if, however, the *negative pole,* with its filament of wood, be rotated, the spots on the *positive pole obey it,* and rotate also. The insertion of a non-conductor, such as a strip of glass, in front of the projecting wooden end, obliterates the luminous spot on the positive pole. When the author first discovered this, he, seeing apparently pieces of dirt on the positive pole, wiped it clean with a silk handkerchief, but there they remained in spite of all wiping; he then examined the negative pole, and discovered a minute speck of dirt corresponding to the luminous spots on the positive pole. When the filament of wood is removed from the negative pole there is sometimes a luminosity or glow over a large portion of the surface of the positive ball. If in this state three or four little pieces of wax, or even a drop or two of water, be placed upon the negative pole, corresponding non-luminous spots will be found upon the positive pole, which rotate with the former, but do not with the latter. It is therefore evident that there are lines of force existing between the two poles, and by these means one is able to telegraph *from* the negative *to* the positive pole to a distance of 8 inches through the air, without any other conductor than that which the electrical machine has constructed for itself across the non-conducting gas. The foregoing seems to the author to give a possible explanation of "ball-lightning;" if it be possible for there to be a negatively electrified cloud sufficiently charged to produce a flash from the earth to the cloud, a point in the cloud would correspond to the wood projection on the negative conductor; if such a cloud exist, a luminous spot would be seen moving about the surface of the earth, corresponding to the moving point of cloud over it, and thus present

phenomena similar to those described by the privileged few who have witnessed this extraordinary natural phenomenon. The following experiment shows that, prior to the passage of the electric spark, a channel is prepared for this spark to pass. The positive and negative balls of the machine were separated to a distance of 6 or 7 inches, and a common candle-flame was placed midway between them. On rotating the machine, the flame was drawn out on each side just prior to the passage of the spark. Sometimes it extended to a width of 5 or 6 inches; this took place every time the spark passed. It is well known that the duration of this spark is less than the $\frac{1}{1000000}$ part of a second; the flame occupied the $\frac{1}{8}$ or $\frac{1}{10}$ part of a second in flying out to make the conducting channel through which the discharge went. The author has been informed more than once by captains of vessels, that when men have been struck by lightning a burn has been left upon the skin of the same shape as the object from which the discharge flew. In one instance he was informed that some brass numbers, attached to the rigging from which the discharge passed to the sailor, were imprinted upon his skin. It is now seen that this is perfectly possible if the discharge be a negative one, that is, if the man be + to the brass number.

Ethnological Society, January 10.—Prof. Huxley, President, in the chair.—Mr. Francis Hewitt was announced as a new member.—A collection of stone implements from Queen Charlotte and Vancouver Islands was exhibited by Dr. Hooker, C.B., and some artificially distorted skulls from Vancouver Island were exhibited by Col. A. Lane Fox.—A paper was read, "On the Prehistoric Monuments in Brittany," by Lieut. S. P. Oliver, R.A., illustrated by a large collection of drawings and plans. The author first described the alignments at Carnac, which are arranged in three distinct groups—those of Menec, Kervario, and Kerlescant. At Menec there are eleven lines of stones with a circle at the south-west; at Kervario there are ten convergent lines; and at Kerlescant thirteen lines, with an enclosure of horse-shoe form. The stone avenues of Erdeven, St. Barbe, St. Pierre, and Plouhinec, were also noticed among the antiquities of the Morbihan; and attention was then directed to the alignment of Crozon, in the neighbouring Department of Finisterre. From the rude character of the stones in all these avenues and circles as compared with those in the neighbouring dolmens and menhirs, the author believes that the megalithic remains are of two distinct classes, differing considerably in date, the alignments and circles of amorpholiths being referable to a much earlier race than the dolmen-builders. Many of the tumuli of Brittany were then described, and attention directed to the archaic sculpturing upon some of the stones, and to the celts which have been found within the tumuli. Of these celts a large proportion are formed of the rare mineral *fibrolite*. The discussion upon this paper was sustained by Sir H. Dryden, Capt. Godwin-Austen, the Rev. H. Winwood, and Messrs. A. W. Franks, Hyde Clark, and Edgar Layard.—Some notes were read on a Cairn near Cefn, St. Asaph, by the Rev. D. R. Thomas, M.A., and T. McK. Hughes, M.A. The authors described two chambered tombs within the area of a large cairn which had long been removed. One of these cists was brought to light by excavations, which were undertaken in consequence of a visit to the spot by a party from the British Association after the Liverpool meeting.

NORWICH

Norfolk and Norwich Naturalists' Society, December 20.—A box of very beautiful and rare fen moths, presented by the Hon. T. de Grey to the Museum, was exhibited.—Mr. Stevenson read a paper on "The Abundance of Little Gulls on the Norfolk Coast in the Winter of 1869-70." This bird has hitherto been considered an occasional straggler on the Norfolk coast, but during the month of February last an unprecedented number appeared. It is probable that over sixty specimens were killed in this county; of these the great majority were adult males. These birds have recently been discovered to breed in considerable numbers in Lake Ladoga, further to the north and west of which it is not at present known to breed. Mr. Stevenson presumes that the stragglers, mostly young birds, which in autumn and winter appear on the coast of Great Britain, form part of that colony, which, migrating in a westerly rather than in a southerly direction, pass from the Baltic into the North Sea. The adult birds rarely approach the shore, but, from the sudden and irresistible force of the gale which visited us in February last, they were, doubtless, compelled to seek the shelter of our bays and estuaries.—Mr. Barrett stated that, since he read his paper on "The Coast Insects found near Brandon," at the last meeting, he had received some valuable evidence confirmatory of the views he then expressed. The Hon. T. de Grey informed him that he had taken *Agrestes cinerea* and *Gelechia vitella*, both of them rare sandhill insects, at Brandon, and *Gelechia marmorea* as far away as Tottington, on the Merton estate. Other species had been taken by the Rev. H. S. Mariott, of Wickham Market, and the Rev. H. Williams, of Croxton.—Mr. Geldart stated that *Phleum arenarium*, an essentially sea-side grass, was to be found growing on the sandy warrens about Brandon.

DUBLIN

Royal Geological Society, January 11.—"On the Geological age of the Ballycastle Coal-field, and its relation to the carboniferous rocks of the West of Scotland, by Mr. E. Hull, F.R.S." The object of the paper was to prove that the coal-field of Ballycastle, Co. Antrim, was referable to the type of the lower coal-field of Scotland, and consequently of the age of the Lower Carboniferous series; in other words, of the mountain limestone. The Carboniferous series of Ballycastle which had been described in 1829 by Sir R. Griffith, F.R.S.,* was shown to consist of three divisions in descending order. 1. *The Upper*, consisting of massive sandstones, and shales with beds of coal, black band and clay band ironstones, &c. (*Lingula squamiformis*). 2. *The Middle*, consisting of a thin bed of limestone lying between shales with carboniferous limestone genera and species of shells, crinoids, and corals. 3. *The Lower*, consisting of massive reddish grits, and conglomerate with thin beds of shale. The author showed that the carboniferous limestone of Ireland undergoes, in its extension northward, changes similar to those of the same formation in Britain, when traced from Derbyshire into Northumberland and Scotland. The calcareous element thins away, and is replaced by sedimentary strata of sandstone, shale, &c., with approximately terrestrial conditions, productive of coal and ironstone. It was thus that in the case of the Glasgow coal-field, the limestone of Derbyshire, several thousand feet in thickness, was represented by only thin bands of earthy limestone, interstratified with a thick series of grits, shales, &c., with ironstone and coal. In a similar manner the Ballycastle coal-field, with only a few feet of limestone, shown in the cliffs of the bay, was the representative of the carboniferous limestone of the centre of Ireland, nearly 3,000 feet in thickness. Mr. Hull regarded the lower division (No. 3) of the Ballycastle beds (as above described) as undoubtedly the representative of the "calciferous sandstone series" of the Geological Survey, which lies at the base of the carboniferous rock of the West of Scotland, and that the middle and upper division (Nos. 2 and 1) corresponds to the carboniferous limestone series, or lower coal-field of that country. As regards the palæontological evidence, it was in favour of this view, as far as it had been studied. Out of thirty-three species observed in the limestone band of Ballycastle Bay, 50 per cent. had been described in the Lower Carboniferous rocks of the West of Scotland,† and one of the uppermost seams of coal, lying above the limestone, had yielded *Lingula squamiformis*, a form characteristic of the limestone series in the North of England, Scotland, and Ireland. Mr. W. H. Baily, F.G.S., concurred in the view of the age of these beds on palæontological grounds. The author concluded by pointing out several features of similarity between the Ballycastle beds and the lower coal series of the West of Scotland, such as the occurrence of several beds of "black band" ironstone; the hydraulic and earthy character of the limestone of Ballycastle Bay, exactly resembling the "Arden" and "Cowglen" bands of Glasgow. Some uncertainty still remained whether there were any beds in the Ballycastle district as high in the Geological Series as the millstone grit, a true coal measure, but until more light could be brought to bear on this question by further exploration, and a comp ete investigation by Government surveyors, the author meanwhile regarded the whole series as Lower Carboniferous.

MONTREAL

Natural History Society, Nov. 28.—Dr. Dawson, president, in the chair. A paper was read upon the Canadian phosphates of lime in their application to agriculture, by Mr. Gordon Broome, F.G.S., calling special attention to the valuable apatite deposits of the Rideau district. The author gave a large amount of valuable information upon the manufacture of "superphos-

* Report on the Coal Districts of Tyrone and Antrim, addressed to the Royal Dublin Society, 1829.

† Trans. Geol. Soc., Glasgow.

phates," noticing the good work being done at Brockville in this particular branch of industry, and proving that large areas of land now wholly or partially exhausted, in various portions of Lower Canada, might still have been yielding large returns of wheat and other cereals, had they been farmed upon a rational system of agriculture, coupled with the judicious employment of phosphatic and other fertilisers. We quote from the concluding words of this comprehensive essay: "He made especial reference to the comparative dormant state of this, and other equally obvious sources of industry in Canada, arising from a great deficiency in a most important division of our national education, and that nothing save a liberal augmentation of the ordinary courses of instruction in modern subjects can ever dispel the immense existing clouds of ignorance and prejudice. It is, therefore, sincerely to be hoped that the very able remarks recently made by Principal Dawson upon this question may have their desired effect, and that Canada may speedily obtain a share in the improvements that have of late almost revolutionised the systems of educations in the universities of the mother country." Dr. Hunt, Vice-President of the Association, then referred in a feeling manner to the loss sustained by science in Canada, in the death of Mr. Hartley, of the Geological Survey, who, though only twenty-three years of age, was one of the most promising young men in the country.

VIENNA

I. R. Geological Institute, December 20.—M. Fr. v. Hauer stated that the Institute had happily come into possession of the only known specimen of Psephophorus polygonus from Neudörfl in Lower Austria, which had been described after a drawing more than twenty years old by Herm. von Meyer, and had been designated by him one of the most interesting fossil remains of a vertebrate animal ever found in Austria. It consists of a plate of epidermal scales nearly two hundred in number, and perfectly irregular in size and form. H. v. Meyer thought it to belong to an animal of the family of the Dasypodidæ, whilst Prof. Müller compared an analogous plate found in the Zeuglodon beds of North America with the plate of Dermatochelys.—M. F. v. Vivenot read a memoir on the microscopical examination of the syenite of Blansko in Moravia, in which, besides the minerals which are distinguishable with the naked eye, as oligoclase, quartz, amphibole, biotite, titanite, and magnetite, he discovered also orthoclase, epidote, and apatite.—M. E. Tietze exhibited specimens of cornubianite from Servia, which perfectly resembled the cornubianite of Cornwall, and like it were found in a stratum interposed between granite and argillaceous schist.—M. Posepny communicated his observations on the beds of rock-salt in Transylvania.

BOOKS RECEIVED

ENGLISH.—The Transformations (or Metamorphoses) of Insects: P. M. Duncan (Cassell and Co.)—Wonders of the Human Body: A. Le Pileur (Blackie and Son).—Dogs and their Doings: Rev. F. O. Morris (Partridge and Co.).—The Genesis of Species: St. George Mivart (Macmillan and Co.).—Matheran Hill: its People, Plants, and Animals: Dr. J. Y. Smith (Maclachlan and Stewart).

FOREIGN.—Vorschule der Experimentale Physik: H. F. Weinhold (D. Nutt).

PAMPHLETS RECEIVED

Prospectus of Sir Joseph Whitworth's Scholarships for Mechanical Science, 5th ed.—A Letter to the Duke of Argyll on the Proposed Scheme for an Engineering College: Dr. F. T. Bond.—On Equal-surface Projection and its Anthropological Applications: C. Piazzi Smyth—On the Cellular Structure of the Red Blood-corpuscle: Dr. J. G. Richardson (Philadelphia).—Report on the Canadian Phosphates: G. Broome, pt. 1.—Eloge de Jean-Theodore Lacordaire: par E. Morren.—Das Differentialphotometer u. eine neue Thermosäule: Prof. K. W. Zenger.—Chemische Notizen: Prof. F. Stolba.—Ueber Das Kieselfluorammonium u. Kieselfluornickel: Prof. F. Stolba.—Ueber Erzeugung algebräischer Curven durch mehrdeutige Elementargebilde: Dr. E. Weyr.—Studien zur Theorie der Covarienten u. Invarienten der binären Formen: Dr. J. Dienger.—Ueber den Zusammensetzen verschiedener Transformationsformeln für elliptische Integrale: Prof. J. Lieblein.—Ueber fortgesetztes Tangentenziehen au Curven dritter Ordnung: Dr. H. Durége.—Beobachtungen ueber einige fossile Pflanzen aus dem Steinkohlenbecken von Radnic: C. Feistmantel.—Beobachtungen über die Entstehung einiger sphäroidischer Gebilde im Mineralreiche: C. Feistmantel.—Die Porphyre im Silurgebirge von Mittelböhmen: C. Feistmantel.—Die Steinkohlengebilde in der Umgebung von Radnitz in Böhmen: C. Feistmantel.—Beiträge zur Lehre der universellen Summirung von Strecken: Dr. W. Martzka.—Die Sätze von Bürmann u. Lagrange: Dr. J. Dienger.—Ueber die Normalenfläche zum dreiaxigen Ellipsoide: J. M. Sölin.—Ueber die Callianassen der böhmischen Kreideformation: Dr. A. Fritsch.—Ueber die physikalischen Constanten des Wasserdampfes: Prof G. Schmidt.—Ueber die neueren physiologisch-psychologischen Forschungen im Gebiete der menschlichen Sinne: Dr. J. Dastich—Auflösung der Gleichungen des 2ten, 3ten, u. 4ten Grades: J. Machometz.—Beiträge zur Weddle's Methode der Auflösung numerischer Gleichungen: J. Popper.—Beiträge zur Auflösung höherer Gleichungen: Dr. J. P. Kulik.—Ueber das Bourdonsche Metallbarometer: Dr V. Pierre.—Ueber das Binocularsehen: Dr. J. Ritter von Hasner.—Die Methode der orthonogalen Projekzion auf zwei Ebenen: Prof. R. Skuhersky.—Parallèle entre les dépôts Siluriens de Bohème et de Scandinavie.—Resultate mehrjähriger Beobachtungen über jene Pflanzen deren Blumenkronen sich täglich periodisch öffnen u. schliessen: K. Fritsch.—Grundzüge einer Meteorologie für den Horizont von Prag: K. Fritsch.

DIARY

THURSDAY, JANUARY 19.

ROYAL SOCIETY, at 8.30.—On the Structure and Development of the Skull of the Common Frog: W. K. Parker, F.R.S.—Method of Measuring the Resistance of a Conductor: H. Mance.—Modification of Wheatstone's Bridge to determine the Resistance of a Galvanometer Coil from a single Deflection of its own Needle: Sir W. Thomson, F.R.S.

SOCIETY OF ANTIQUARIES, at 8.30.—The Pre-Christian Cross; H. M. Westropp.

LINNEAN SOCIETY, at 8.—On the Vegetation of the Solomon Islands: Mr. Atkin.—Note on Byrsanthus (*Homalineæ*): Dr. M. T. Masters, F.R.S., F.L.S.—Historical Notes on the *Radix Galanga* of Pharmacy: Daniel Hanbury, F.R.S., F.L.S.

CHEMICAL SOCIETY, at 8.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

FRIDAY, JANUARY 20.

ROYAL INSTITUTION, at 9.—Dr. Tyndall.

SATURDAY, JANUARY 21.

ROYAL INSTITUTION, at 3.—Laws of Life revealed in History: Rev. W. H. Channing.

ASSOCIATION OF OFFICERS OF HEALTH, at 8.—On Parasites, in relation to the Sewage Irrigation Question: Dr. Cobbold.

SUNDAY, JANUARY 22.

SUNDAY LECTURE SOCIETY, at 3.30.—How Coal and the Strata in which it is found have formed: A. H. Green.

MONDAY, JANUARY 23.

LONDON INSTITUTION, at 4.—On the First Principles of Biology: Prof. Huxley (Educational Course).

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.

ENTOMOLOGICAL SOCIETY, at 7.—Anniversary Meeting.

TUESDAY, JANUARY 24.

ROYAL INSTITUTION, at 3.—Nutrition of Animals: Dr. Foster.

ETHNOLOGICAL SOCIETY, at 8.—On the Languages and Tribes of East Africa: Rev. Dr. Steere.—On African Weapons and Implements: Dr. Eyschmacher.—A Zulu Law Case: Sir John Lubbock, Bart., M.P.

WEDNESDAY, JANUARY 25.

SOCIETY OF ARTS, at 8.—On New Paper-making Materials, and the Progress of the Paper Manufacture: P. L. Simmonds.

LONDON INSTITUTION, at 7.—Dust and Disease: Prof. Tyndall. (Conversazione.)

GEOLOGICAL SOCIETY, at 8.

THURSDAY, JANUARY 26.

ROYAL, at 8.30.

SOCIETY OF ANTIQUARIES, at 8.30.

LONDON INSTITUTION, at 7.30.—On the Action, Nature, and Detection of Poisons: F. S. Barff.

ROYAL INSTITUTION, at 3.—Davy's Discoveries: Dr. Odling.

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